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Alexander Philips Wilson Philip (1770-1847)

Physiologist and Physician*

WILLIAM H. McMENEMEY**

IN the annals of medicine there are very few men who, lacking the amenities and security of an academic post, have found time for both practice and experimental research. Such a man, however, was Alexander Philips Wilson Philip, who in his day was regarded as a foremost if controversial physiologist, a competent pathologist, and a successful and fashionable physician. His prolific writings testify to a busy and varied life in practice, so there can be no doubt as to his industry. Certain personal failings, however, which will become apparent as the story unfolds, served to deprive him of the respect which might have been his due. His death passed unheeded: no obituary notice was written.[1]

His grandfather, Alexander Wilson, was a merchant who inherited the estate of Shieldhall,[2] Renfrewshire, from the great grandfather John Wilson[3] of Glanderston, a Glasgow merchant and an early but unsuccessful explorer of the coalfield of the Clyde. Nothing is known of his father, also Alexander Wilson, and his mother, whose maiden name was Agnes Gillespie, other than that they were the parents of a second child born in 1773 and christened Philip Wilson. Shieldhall was sold by the creditors of Alexander Philip in 1781 but whether the insolvency concerned the father or grandfather is not known. The subject of this biography changed his surname from Wilson to Philip when he succeeded to the chieftanship of his clan on the death of his paternal grandmother, Susannah Wilson, in 1811. She was the daughter

* Based on a communication to the Royal Society of Medicine, London, on 1 February 1956.

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1 Munk (1886) and Payne (1896) wrote short biographies, while within recent years Fulton (1931), Franklin (1933), and His (1949) have referred to his work.

2 Shieldhall was sold to John Wilson by Bailie Thomas Hamilton, maltman, who built it about 1720 (T. C. F. Brotchie, "Shieldhall," *Glasgow Evening Times*, 22 Oct. 1910).

3 Mr. A. B. Paterson, City Librarian, at the Mitchell Library, informs me that there is listed (item 16) in *The Memorial Catalogue of the Old Glasgow Exhibition 1894*, a portrait lent by the City Parochial Board, of this same John Wilson by an unknown painter. He must have prospered for he bequeathed to the Town's hospital £450, a sizeable sum in those days. He was born in 1693 and died in 1786.

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and heiress of John Philip,[4] governor of the island of St. Martin (now part of Guadeloupe) and laird of Almeriecloss, Angus: she incurred her father's displeasure when, in 1741, she married Alexander Wilson in St. Martin.

Wilson Philip was born at Shieldhall on 15 October 1770 and baptised "Alexander Philips, lawful son" to his parents, on the 27th of the same month in the parish of Govan. He was educated at the Edinburgh High School where he studied under Alexander Adams, the Roman antiquarian. From the age of twelve the boy was fortunate enough to have his education superintended by William Cullen and in due course proceeded to the University of Edinburgh to become the pupil of James Gregory, Alexander Monro *Secundus,* Joseph Black, Andrew Duncan, James Home, and Daniel Rutherford. On 16 December 1791 he was admitted to fellowship of the Royal Medical Society and on 25 June 1792 acquired his doctorate, his thesis being entitled *An experimental essay on the process of digestion.* In the same year he published, with a charming dedication to Joseph Black and Alexander Monro, *An inquiry into the remote causes of urinary gravel,* in which he refuted the purely chemical view of the process put forward by François Magendie. His interest in these subjects dated back to the time when, as a student, he awoke one morning with a severe rheumatism of the head and right shoulder which kept him in bed for five weeks. This observant student noted that although his diet consisted largely of butter and calves foot jelly, much lithic acid was being deposited in his urine.

When he was recovered sufficiently to leave his bed he found himself in a state of weakness and great anxiety which persisted for four days. Feeling parched and nauseated, he sucked an orange hoping that it would bring back his appetite, but for the whole of the next day he continued to feel sick. In desperation, there-

4 "The Hon'ble John Philip, Lord of Almery-Closs and Governor of the Island of St. Martin" lies buried in the graveyard of the Dutch Reformed Church at Philipsburg, the town he founded.

My friend, Mr. E. van Tongeren of Amsterdam, informs me that the story of his remarkable life is contained in A. Knappert's *Geschiedenis van de Nederlandsche Borenwindsche Eilander in de 18 eeuw* (Gravenhage, 1932) and in the Rev. Dr. Kraft's *Oranje en de zes Caraebische Parelen* (Amsterdam, 1948). Born in 1688 he was in 1730 appointed Captain-Lieutenant of the "burghers of St. Maartens" by the directors of the Dutch West Indies Company but was driven off the island in 1736 by the natives who regarded him as rude and dictatorial. He returned to Europe and visited Scotland in 1744. He then went west again, was captured and robbed by Spanish pirates, and put ashore at Haiti without even his shoes. He eventually found his way to St. Martins and was accepted by the natives at a time when the island was in danger because of war between England and France. He rebuilt Fort Amsterdam and "died of disappointment" in 1746 because his company had refused to accept his advice to occupy the French portion of the island.

fore, he tickled his throat to induce vomiting when, to his surprise, yesterday's orange juice was returned unaltered. Why was this? He must, he argued, have been lacking in gastric juices and his anorexia must surely be due to this deficiency. He initiated, therefore, a series of experiments to study the character of the gastric juice in anorexia and dyspepsia. On one occasion, having fasted for 17 hours, he sharpened his appetite by exercise and then made himself vomit. When he was rid of the juice, which he found to be ropey exactly as he had seen it in the stomach of a cow,[5] he found he had lost his appetite.

His researches on urinary output were mostly made on himself, but some were carried out on a 15-year-old boy patient "who enjoyed all the experiments": others were on a healthy young gentleman of his acquaintance who was receiving mercury for "a slight syphilitic affection." He tried the effects of varying the diets and regimens, noting in particular any changes in the urine brought about by the addition of lemon juice to the diet. In one experiment he appears to have festooned himself around the neck with litmus paper in an attempt to alter the reaction of the sweat. He concluded from his studies that gravel generally originated from a precipitation of lithic acid in the kidneys. As a preventative he recommended attention to the diet, exercise, and free action of the bowels, measures which he claimed tended to remove and prevent dyspepsia and to support a due action of the skin. He advised the use of antacids and in this connection thought that soap was important for the skin.

For the treatment of dyspepsia this youthful physician recommended exercise, cold bathing, and "usus modicus veneris": this last remedy, the recommendation of which must have profoundly shocked the more sedate and puritanical members of the faculty, he had seen attended with sudden and remarkably good effects "when almost every other remedy had failed. . . . It may be proper in circumstances when it can be made use of, to employ it," he wrote, adding ". . . it is to be regretted that it can so seldom be used with propriety; and that when it is, it is often carried to such excess that it proves more frequently the cause than the cure of dyspepsia." He concluded: "We may say of this remedy what we can hardly do of any other recommended by physicians, that it only requires to be less agreeable to the patient to answer every

[5] In his *Treatise on indigestion and its consequences* (1821) the animal is referred to as a crow.

intention for which it is designed." In later publications he makes no reference to this novel and unconventional cure for dyspepsia.

About this time, but it is not clear when, he signed on as a physician's pupil at St. George's Hospital[6] and attended the lectures of Dr. Matthew Baillie. However, by 1794 he was back in Edinburgh where on the 1st of February he spoke at the Royal Medical Society. Sensation, he told them, existed in the brain and not in the periphery of the body, as was clearly apparent from the well-known phenomenon of the phantom limb: sensations were referred to the parts on which oft-repeated stimuli left their imprint. On 3 February 1795 he was admitted a Fellow of the Royal College of Physicians in Edinburgh.

Since his return to Edinburgh he had been engaged in practice and in pursuing his experimental researches. His *Experimental essay on the manner in which opium acts on the living animal* was published in 1795.[7] The subject had intrigued him ever since he had seen a man drink four ounces of laudanum without perceiving from it even a tendency to sleep. His experiments, made on frogs and rabbits, although crude, were ingenious, but he was always careful to avoid causing suffering to the animals. He introduced his account of the experiments with a critical and somewhat disparaging review of previous work by others: "I shall merely collect together," he said, "the few seeming facts which remain after rejecting the uselessness and inconsistent observations in which they have been involved by the ingenuity of authors and then particularly for particular opinions." Wilson Philip reveals himself as an admirer of the techniques of Felice Fontana,[8] and as a staunch supporter of Monro, whose lectures he had attended and noted down in 1789. Perhaps it was for this reason that he was so critical of his colleague William Alexander who, in his thesis for doctorate,[9] had opposed the view of Monro on the action of opiates. He manifests "some surprise in seeing a man of Dr. Alexander's ingenuity permit himself to be so evidently misled." Monro's experiment had consisted of pouring thirty drops of a strong solu-

[6] Miss Jessie Dobson informs me that his name does not figure on the list of St. George's pupils in the possession of the Royal College of Surgeons. It may, however, be incomplete. She informs me also that there are no references to him in the Hunter-Baillie correspondence in the possession of the College.

[7] The copy in the possession of the Royal College of Physicians is inscribed to Dr. Bayley. This name does not figure on Munk's *Roll*, but a John Bayley "Anglo-Brit" qualified in Edinburgh in 1757.

[8] Abbate Felice Fontana (1739-1805) tended to be lavish in his use of animals: on one occasion he used 300 frogs.

[9] *De partibus corporis animalis quae viribus opii parent*, Edinburgh, 1790.

tion of opium through a hole in the peritoneum and then observing the quickening beat of the heart followed by general convulsions. He had repeated these experiments after first cutting out "the ventricle of the heart" and found that the animal still went into convulsions but took somewhat longer to do so. Monro concluded, as Whytt had done earlier, that the opium was transmitted to the nervous system by way of the nerves. However, he had also proved that drugs were conveyed from their point of application to distant parts by way of the blood stream, for he found that severance of the nerves made little difference to the time of onset of the effects of the drug.

This was the state of affairs when Wilson Philip set to work to confirm Monro's findings. On introducing a solution of opium into the heart he perceived that it passed along the aorta towards the brain. Why, then, could not convulsions be accounted for by a direct action of the drug in the blood on the brain? What need was there to postulate a sympathy of the nerves of the heart? If he severed the aorta, the animal had no convulsions which surely proved that the opium reached the brain via the blood stream. He observed the web of a frog's foot under the microscope[10] and noted that the circulation became languid when eight to ten drops of the opium solution were placed under the skin. The drug, he concluded, had a direct effect on the blood vessels. He naturally took care not to compress the foot and its delicate vessels against the stage of the microscope. His final set of experiments was designed to test the effect of opium when applied directly to the brain: on this occasion he found that convulsions occurred and the muscles went into repeated spasms even though the heart beat remained undisturbed. He therefore concluded that opium had a direct effect on the brain but that it normally reached it through the mediary of the blood stream. He found that exactly the same happened with tobacco. He was, in this instance, unduly modest in his claim, "I am very far from asserting that on these accounts these results ought to be granted; especially when they contradict the inferences which we must draw from the experiments of a Whytt, a Monro or a Fontana," he wrote.

One subject led to another. His association with William Cullen stimulated an interest in fevers, and after spending all of his available time in the study of the literature, he was ready by the

[10] Hastings (1820) gave him the credit for being the first to study the phenomenon of inflammation under the microscope.

summer of 1796 to deliver a course of lectures, which he illustrated whenever possible with physiological experiments. In 1797-98 he was one of the presidents of the Royal Medical Society.

One would have thought that an assured future awaited him in Edinburgh both in practice and teaching. But we read in Pettigrew's biography of Wilson Philip, an account which seems to have met with the latter's approval, that in 1799 because of failing health he considered it advisable to leave Edinburgh and he settled in Worcester. He himself says:[11] "a very infirm state of health has obliged me to abandon the plan of continuing to give lectures." Callisen, however, records that he first went to Winchester, and this view has been followed by Munk. Enquiries[12] reveal that he was actually appointed physician at the Winchester Infirmary on the recommendation of a subcommittee which met on 7 March 1798. The general court of governors on the 4th of April confirmed his appointment and also that of Dr. Joseph Barker. Six days later he was elected a governor: the records of the hospital, however, do not show that he ever attended a meeting. If he spent any time in Winchester at all, it must have been in lodgings, for his name does not figure on the rate books; nor did he attend the poor, who at that time were numerous because of the heavy influx of French refugees.[13] His four-volume book on febrile diseases, however, was published there between 1799 and 1804 and his essay on the *Use and abuse of mercury* in 1805, but only in the first volume of the former did he inscribe himself as "Physician to the County Hospital of Winchester."

One naturally wonders why, after election to the staff of the Winchester Infirmary, he abandoned it for the chance of a vacancy at Worcester Infirmary. Was Worcester potentially a more lucrative centre for a man who proposed to carry the gold-headed cane, or was it climatically more suitable for an ailing man? He appears to have been in Worcester as early as 1799 when he applied for the vacancy on the staff occasioned by the resignation of James Johnstone. However, he did not gain the necessary support.

His move from Winchester may have been due to his health, to his dissatisfaction with the professional standing of his colleagues there—for he would find it a very different life from Edin-

11 Wilson. A. P. *A treatise on febrile diseases.* Vol. I, Winchester, 1799, preface.

12 I am grateful to Mr. F. R. Reeve, Secretary of the Royal Hampshire County Hospital, Winchester, for the information which follows.

13 Mrs. W. J. Carpenter Turner, Archivist to the City of Winchester, kindly undertook the search.

burgh—or he may have been invited to move to Worcester, for it is conceivable that James Johnstone with his two surviving doctor sons settled in Birmingham would look for a fellow Scot to succeed him: James Johnstone, it will be recalled, was also interested in physiology, especially of the sympathetic nervous system. But why did Wilson Philip, when unsuccessful in the election of 1799, not apply for the next vacancy which occurred in 1801? Perhaps he was too ill, or perhaps he had ambitions of succeeding Daniel Rutherford in Edinburgh in 1802. However, he applied for the next Worcester vacancy in 1802 and on this occasion he was successful.

Meanwhile, his *Treatise on febrile diseases* was becoming widely known. Dedicated to Matthew Baillie, it went through four editions and was translated into German and French. Today it makes tedious reading and contains little of interest to the historian. It is an exhaustive account of the fevers known at that time and it reveals a profound knowledge of the medical literature. Quotations are numerous, but case reports are few: indeed, the author prefers to make use of generalisations. Inflammation, he held, was the only local disease which could excite fever. Haemorrhage, and profluvia, believed by Cullen to be independent causes of fever, excited fever, he said, only in proportion as they were attended with inflammation. He admitted the periodic nature of some fevers but could not account for them. He could not agree with Dr. Henry Clutterbuck that fever was always supported by a determination of blood to the brain. The first volume did not receive the welcome he expected so in the preface to the second he set out to vindicate himself. He answered charges of being too diffuse, too encyclopedic, and too theoretical: he could not please everyone, he wrote rather testily. The reviewers had clearly incurred his displeasure.

Worcester proved to be a suitable culture medium for Wilson Philip's peculiar temper. Since 1794 there had been a simmering feud between the physicians and the surgeons in which the apothecaries mostly sided with the former. The surgeons were accused of undertaking the care of medical cases, but they defended their rights claiming that they were not "pure" medical but "mixed" cases, and these they had a right to treat. There was a significant flare-up of this feud soon after Wilson Philip's arrival in Worcester, and for some years the physicians and surgeons were not on speaking terms.

In spite of this, Wilson Philip seems soon to have become a successful physician and was able to turn an enquiring eye, as John Wall had done before him, towards the Malvern Hills and their potentially lucrative springs. He was impressed with the fact that many chalybeate springs containing but a trace of iron were more efficacious in restoring vigor than the most powerful of artificial chalybeates. He carried out a careful analysis and published his results in 1805, sending a copy[14] to Martin Wall who in 1780 had analysed the waters and republished his father's monograph.

The year 1805 also saw the publication of his *Observations on the use and abuse of mercury,* which was addressed to the public. The common mistake, he held, was that the drug was administered in too large doses: for this reason it was getting into a disrepute which, because of its usefulness when properly administered, was undeserved.

Two years later there appeared an essay on *The nature of fevers* in which he set out to ascertain the principles of their treatment. It is verbose, largely theoretical, but nevertheless competent and critical. Fever might act, he thought, on the heart and large blood vessels, or on "the circumference," by which he seems to have meant the capillaries. Treatment should aim at avoiding excitement of the animal organs, but at the same time the circumference of the vital system had to be excited. He seems to have been aware of the condition of peripheral vascular failure.

He appears to have been a kindly doctor, always thoughtful for his patient's welfare and up to date in his treatment. For instance, there lived ten miles out of Worcester an old gentleman who had lost a leg in the American campaign. His complaint was one of dyspnoea. Wilson Philip was called in by the local apothecary and so was Dr. Edward Johnstone[15] of Birmingham. Together they prescribed stomachic draughts with vitriolated iron and occasional anodyne draughts. Two nights later when alarming symptoms supervened, Wilson Philip was recalled. On the following day Dr. Johnstone came out for a further consultation. They agreed that the outlook was hopeless but they made "cautious trials of the foxgloves in anodyne draughts." They prescribed also ipecacuahana and rhubarb to excite the bowels. Wilson Philip remained all night with his patient, who two days later

14 Now preserved in the Malvern Public Library.

15 Edward Johnstone (1757-1851) was the son of James Johnstone the elder (1703-1802) of Worcester.

was bled by the apothecary. As his condition became increasingly more severe, Wilson Philip wrote about him to Matthew Baillie who advised repeated emetics. But there was no improvement until the chance arrival of a doctor friend of the patient named Charlton who had been surgeon to the late Duke of Gloucester. He advised and prescribed James's powder with opium, which was taken with great benefit. The patient, however, died seven weeks later, when the full story and the post-mortem findings were sent to Matthew Baillie for communication to the Medico-Chirurgical Society.[16]

Wilson Philip wrote profusely but not in an easy style: indeed, many of his writings and especially those in his later years are difficult to follow. However, it was entirely due to those writings that the Worcester Infirmary was known outside its county. Indeed, it is probable that it was he who founded the local medical society which, for the purpose of lending books at any rate, was in existence in 1806.

In 1811 two important events took place in his private life. On the 15th of February[17] his surname was changed by Royal Licence from Wilson to Philip, when he succeeded on the death of his paternal grandmother, Susannah Philip, to the chieftanship of his clan[18] and to property in Scotland, described in the records of the College of Arms as being "considerable". It was, says Pettigrew, one of those occasions when titles may be transferred on the female side. Wilson Philip was proud of his grandmother's family. A photograph[19] of "Almeriecloss Mansion House" before

16 On 8 Sept. 1812 (*Trans. med.-chir. Soc.*, 1812, *3*, 290).

17 *Berrow's Worcester Journal*, 5648, 7 March 1811.

18 The progenitor of the Almeriecloss Philips was Henry Philips, D.D., who was appointed minister of Arbroath in 1601 and married Isabella Petersone, dying in 1627. His son James, who died in 1634, was described in deeds as "burgess and bailie of Aberbrothock." The grandson, James Philip II, who took up the titles in 1653, is supposed to have built Almeriecloss House which was demolished in 1931: the name is a corruption of Almonrieclose. Fiery tempers seem to have characterised several members of the family, for family bickerings and even court proceedings seem not to have been unusual. One holder of the estate, James Philip III (1654-5 -?1719-?25) was the author of the *Grameid* which narrated the campaign of Viscount Dundee in 1639 and necessitated its author going into hiding after the battle of Killiekrankie. Perhaps the most resourceful member of the family was John Philips (great-great-grandson of Henry Philips) who in 1721 was described as "a merchant of Aberbrothock" and owner of "the Providence" in that port. In 1727 he granted a factory to his brother James to recover debts due to him "as he was going abroad" (see note 4). In 1753 John Philips' only child Susannah, who was then married to Alexander Wilson, completed her title to the Almeriecloss estate. The retour, dated 1 September 1752, also speaks of her as heir of line and of provision of her great grandfather James Philip (II) in land of Keptie, Dishland, and Lordburn, which had been in non-entry since his death in 1695-6. Presumably, Wilson Philip succeeded to these lands also. In 1753 Susannah Wilson sold Almeriecloss to one Robert Barclay (Warden, 1885: McBain, 1897: Hey, 1899: and information kindly supplied by Mr. Norman Crawford, Librarian and Curator of the Art Galleries, Arbroath).

19 Preserved in the Arbroath Public Library.

it was demolished in 1931, shows a disappointingly ordinary and grim-looking, stonebuilt house. However, an earlier residence and estate of this name must have been more important, for the rental roll in the mid-seventeenth century amounted to £450 per annum.

The second event in 1811 was his marriage at St. Mary's Church, Cheltenham on the 25th of March to Mary Domvile,[20] sixth daughter of Charles Domvile, M.P.,[21] né Pocklington, of Santry Court, Dublin, and his wife Margaret Shepard. His marriage into the Irish aristocracy seems to have coincided with peace in the faculty at Worcester, for *Berrow's Worcester Journal* was able to announce: "We are happy to state that the differences between the physicians and surgeons are settled."[22] Agnes, their first-born, was christened on 24 June 1812.

He now began a new programme of experiments which he seems to have carried out in the Worcester Infirmary. They were motivated by the publication of LeGallois's[23] *Expérience sur le principe de la vie,* whose thesis was that the phenomena of life, sensation, and motion were not in so great a degree to be attributed to the cerebrum and cerebellum as physiologists were tending to think, but that the principles of sensation and motion, so far as the truth was concerned, resided, as Galen had supposed, in the spinal marrow. LeGallois believed that the heart's action was maintained by the spinal cord through branches of the sympathetic nervous system. His experiments, which had begun as early as the year 1808, had been deemed cruel but decisive. Wilson Philip, however, decided to repeat them, for he could not reconcile LeGallois's conclusions with his own experiments made on animals with opium and tobacco. The arrival in 1812 of an industrious new apothecary house-surgeon in the person of Charles Hastings must have greatly pleased Wilson Philip who seems at all times to have directed the experiments, allowing others to carry out the operative procedures. They were happily matched, and there is no doubt that Hastings owed much to his enterprising preceptor. In one of his papers[24] Wilson Philip pays tribute to his young assistant whose "expertness in dissection was

[20] W. P. W. Phillimore.*Gloucestershire parish register,* London, 1898, 7, 105; *Gentleman's Mag.,* 1811, *81* (i), 392.

[21] He had died the previous year: his mother was a sister of Sir Compton Domvile, Bart., and a sister-in-law of the 3rd Baron Santry. Wilson Philip, by his marriage, became brother-in-law to Sir Compton Pocklington Domvile, Bart.

[22] 5693, 16 Jan. 1812.

[23] César Julien Jean LeGallois (1770-1814) was physician-in-charge of the hospice and prison of Bicêtre.

[24] *Philos. Trans.,* 1815, *105,* 65.

often of great use when it was necessary to be expeditious and to lose as little blood as possible." These experiments were mostly of the same order, being designed to ascertain the relationship which existed between the cardiovascular and nervous system. They would knock rabbits on the head and remove the spinal cord, sometimes, for the sake of speed, destroying it with hot wire.[25] They proved to their own satisfaction that the heart acted independently of the brain and cord, as indeed Haller had stated: yet paradoxically it could be altered by drugs which acted on the nervous system. These experiments mostly confirmed those of LeGallois, although Wilson Philip believed that the former's deductions had been premature and erroneous. While believing that the heart and blood vessels acted independently of the brain and cord, Wilson Philip held that the nervous system could act as a stimulus to both of them and could on occasions depress them to such an extent as to destroy their power. He did not believe that the depression was merely an excess of the stimulating effect. One of the experiments must have been exciting. Hastings had the web of a frog's hind leg on the stage of the microscope and was actually observing the vigorous capillary circulation when Wilson Philip crushed the brain with a single blow of the hammer. Hastings noted an immediate loss of power in the capillaries with a cessation of the blood flow which, however, gradually resumed although with diminished force. This devastating experiment was varied by making use of the mesentery of a rabbit and the fin of a lampern. In these experiments Wilson Philip seems to have held the view that the characteristic hyperaemia of inflammation was due to debility of the capillaries followed by increased action of the arteries, whereas Hastings, who subsequently pursued his studies at Edinburgh independently, explained the phenomenon as a weakening of the capillaries with a consequent disturbance of the equilibrium between the larger and smaller vessels and a passive distension of the latter. The theories were essentially identical. Wilson Philip clearly believed that the blood vessels possessed a propulsive power which was independent of that of the heart and that the heart and blood vessels were independent of the brain and spinal cord. In a paper to the Royal Society in 1815 Wilson Philip claimed to have proved that the nervous impulse was in fact "the electric fluid." Wilson Philip and Hastings were,

[25] Felice Fontana always picked them out with a pin because decollation was associated with the loss of too much blood.

it will be seen, well up to date in their technique of animal experimentation and by 1812 were using freshly killed animals modelling their technique on that of Le Gallois which had been published four years earlier.

Apart from their animal experiments he and Hastings were busily engaged in assessing the therapeutic value of galvanism. Applying one electrode to the nape of the neck and the other to the pit of the stomach, they found this form of treatment particularly useful both in derangments of the nervous system and in habitual asthma. They took care to obviate the effects of suggestion by making use of controlled observations on the same patient without any current being transmitted. On 21 November 1816 Wilson Philip's paper on the *Effects of galvanism in restoring the due action of the lungs*[26] was communicated to the Royal Society by Sir Everard Home, Vice-President. About this time he seems to have become interested in the subject of dyspeptic phthisis and in 1817 published his *Observations on a species of pulmonary consumption*. Dyspepsia and habitual asthma, he found, were very common symptoms among the sedentary china manufacturers.

When Hastings left in 1815 to pursue his medical studies in Edinburgh, Wilson Philip was assisted by Herbert Cole[27] and by James Pook Sheppard.[28]

Wilson Philip had by now submitted four papers for publication in the Royal Society's *Transactions*. The first two and the last were accepted, but he heard nothing of the fate of the third other than that the President had personally approved of it. Then Wilson Philip learned that the President had been subsequently advised that his conclusions and the observations on which they had been based were deemed by experts within the Society to be incorrect and that for this reason publication had been refused. His contention had been that division of the pneumogastric nerve[29] (called the 8th in those days) was followed by arrest of the process of digestion, but that galvanism would largely, if not entirely, obviate these effects. Wilson Philip requested that since his paper had not been accepted, either it should be returned or a copy be made and sent to him. The Council agreed to the latter course,

26 *Philos. Trans.*, 1817, *107*, 22.

27 Herbert Cole had been house surgeon for thirty-seven years when he died in 1852 at the age of 55.

28 James Pook Sheppard (1784-1853), surgeon to the Worcester Royal Infirmary and joint secretary with Charles Hastings of the Provincial Medical and Surgical Association (later to become the British Medical Association) at its inception in 1832.

29 LeGallois (1812) had already shown that bilateral section of the vagus might produce fatal bronchopneumonia.

and a clerk was ordered to prepare a copy. To Wilson Philip's surprise, however, the account of his experiment differed from the one he had sent and the conclusions were the opposite of his own: he was even more surprised when the clerk explained in a note that the description of the experiment was in another handwriting and that it had been pinned over what clearly had been Wilson Philip's own account. Why had this extraordinary substitution been made in the official files of the Society and why had he not been told? The result of this experiment was the antithesis of his own and the technique employed had been different. He was completely perplexed but decided his best course was to recount the extraordinary business in a book which he had ready for publication entitled *An experimental enquiry into the laws of the vital function*[30] (1817) and trust that the experimenter who had usurped his rightful place in the files of the Society would own up and explain the motive for his strange behaviour. A year lapsed and he heard nothing, so in an appendix to the second edition of this same book he made an appeal to the President and Council of the Royal Society to do what the cause of truth required of them. This work was widely read and translated into French and German. Dr. Matthew Baillie communicated the substance of it to the Medico-Chirurgical Society of London.

Wilson Philip's fame, in spite of the curious silence of the Royal Society, was spreading, but alas he could not keep the peace with his colleagues in Worcester. The junior physician was elderly and lazy so that his retirement in 1816 was hardly noticed. So busy was Wilson Philip that he invited a fellow Scot named Jonas Malden to join him in practice and also at the Infirmary pending an official appointment.

When Wilson Philip heard that it was being whispered abroad that he was the cause of the many dissentions which prevailed in the Infirmary, he instituted careful enquiries to find out who was the author of these false accusations. After investigations spread over a year he succeeded in pinning these rumors on to one man, and at once called a meeting of the doctors to tell them that he proposed to lay the disagreements before the governors. None of his colleagues attended this meeting, so, asking the only other physician to accompany him, he called on the surgeons in their

[30] Mr. L. M. Payne, F.L.A., of The Royal College of Physicians, informs me that of five works by Wilson Philip entered in the *Catalogue of the medical library of the late Matthew Baillie, M.D., F.R.C.P.*, 1813, this is one of three inscribed by the late author: the other two are *Malvern waters* (1805) and *Treatise on indigestion* (1821).

consulting room and told them that he proposed to call a governor's meeting. They received his threat in icy silence, and when the governors' meeting was convened, they refused to attend. What happened at the next special meeting is best described in Wilson Philip's own words:

> There was a party in the room, many of whom were their particular friends who made every effort to stop the proceedings; and would on no account suffer me to go on unless I brought forward a specific charge . . . those who had thus forced me to make the above charge proceeded to censure me in every possible way for having made it. The charge was substantiated among other unanswerable proofs by the direct and indirect acknowledgement of the accused themselves; yet the meeting in opposition to a numerous and most respectable minority proceeded to find it groundless and thanked them for the excellence of their conduct.

Wilson Philip, realising that the judgment of the meeting was founded on a surgical opinion which he believed to be biased, asked them to state the opinion in writing: this they refused to do, neither would they agree to an independent opinion of the surgeons of Worcester, of Birmingham, of Gloucester, or even of the College of Surgeons. He added: "And when I wished further to remonstrate against this and other irregularities, my voice was drowned by the cry of question from my opponents." It must have been an unpleasant occasion: "I heard myself accused of it publicly and in the grossest manner," he later recalled, adding: "The silent contempt with which I received the accusation was my reply to it." On the 22d of April he published his resignation,[31]—"Dr. W. Philip, finding from the precedent established by the determination of the meeting held at the Infirmary on Monday last that he cannot consistently with his views of the nature of such an institution any longer continue with his attendance, here begs leave to relinquish the situation he holds there." His resignation was so hurried that Mrs. Hastings felt constrained to write two days[32] later to the local press on her son's behalf, putting forward the news that he would almost certainly be a candidate for the vacancy on the Infirmary staff. Hastings was duly appointed, and when Wilson Philip moved to London he took over most if not all of his practice.

In a fuller explanation Wilson Philip tells of his endeavour during the earlier years of his stay in Worcester to remedy the shortcomings of the state of affairs in the faculty. The more important centres such as Worcester deserved the services of a con-

31 *Berrow's Worcester J.*, 6018, 23 April 1818.
32 *Ibid.*, 6019, 30 April 1818.

sultant physician, for medicine was too big a subject for any man to encompass the whole of it. But however laudable may have been his wish to see the highest standard of medical ethics prevail in his city, Wilson Philip must have been lacking in tact if he had to quarrel in this way with his colleagues. Indeed, it seems probable that he was somewhat paranoid in his attitude to some of them. That he had inherited the Philip temper is clear on his own admission: "If I have on any occasion spoken with too much warmth; if it be thought that I have too much waived delicacy towards my colleagues, let it be recalled that it was not in my own cause." Nevertheless, he wins our admiration for adhering to his principles. "I have always doubted whether it is proper for any of the medical men publicly to notice even the slightest degree of incapacity in his colleagues," he wrote. "My resignation will better than any other means impress on the governors the various evil effects of the irregularities which prevailed in that house."

Although he resigned from the staff in 1818, he continued his interest in the Infirmary and attended a governors' meeting as late as December 1819. He was fully occupied, no doubt, with what Munk called his "large and lucrative business" at Worcester.

It was on 18 May 1819 and some time after the unpleasant incident at the Worcester Infirmary that Wilson Philip was shown an unsigned article[33] entitled *A few facts relative to Dr. Wilson Philip's attack on the president and council of the Society.* It explained that after the presentation of Dr. Philip's controversial paper many in the audience had expressed a wish that his experiments should be repeated and that three members had undertaken to do this. Wilson Philip's instructions had been carefully followed and, after the experiments had been completed, his original protocols had been returned to the files. Unfortunately, a minute made at the time of the repeated experiments had been accidentally left in the file and this had evidently been copied by the clerk. But the president had been advised that the original experiment was in any case ill conceived because the rabbit was a ruminant and so not suitable for the purpose, while the second lot of experiments appeared, they thought, finally to set the enquiry at rest and to disprove the original experiments of Wilson Philip. It had been intended to lay these results before the Royal Society but "the morbid sensitivity shown by so many members on hearing the experiments detailed by Dr. Wilson Philip, deterred the au-

[33] *J. Sci. Arts*, 1819, 7, 161.

thor from running the risk of so soon again awakening these feelings." Wilson Philip was astonished. Two days later he sent to the same journal an indignant reply.[34] It was quite by chance, he told them, that he had read the article in the preceding number of the journal. Addressing himself to William Thomas Brande,[35] he asked why his perfectly natural request for an explanation should have been interpreted as an attack on the president and council. "I cannot conceive, sir," he wrote, "that you are activated by a wish to prepossess the feelings of the reader against me, before you make an appeal to his judgement." He then proceeds to argue. How could the minute have been *accidentally* left in? Why, it was pinned in! No, his contentions had been deliberately overruled without his having been given a chance of defending himself. He, with Hastings and Sheppard, had examined upwards of 130 rabbits and they knew what they were talking about. The President had certainly been misinformed about the rabbit, for it was not a ruminant, and in any case one of his experiments had been made on a dog and this fact they would have known had they taken the trouble to read *Vital functions.* As for the morbid sensitivity of some of the members simply because some of the experiments had been made on living animals, why this sensitivity was neither morbid nor unreasonable but was due entirely to the fact that reports had been industriously circulated around the members that his experiments were not only useless but had been made on the wrong animals. A certificate from Hastings was appended to Wilson Philip's complaint attesting to the fact that he had cut the vagus more than twenty times and the result had always been as Wilson Philip had said and not as the Society had reported. The Royal Society remained silent and so did the experimenters who remained unknown to Wilson Philip until the publication of Cooke's *Treatise on nervous diseases*[36] early in 1820 in which he referred to the experiments made by Benjamin Brodie following on those of Wilson Philip.[37] So it was

[34] *J. Sci. Arts,* 1819, *7,* 349.

[35] William Thomas Brande (1788-1866), F.R.S., chemist and experimentalist and also editor of the *Dictionary of science and art* (1842-52).

[36] John Cooke (1756-1838) M.D., London, was from 1784-1807 physician to the London Hospital. Only the first volume appeared in 1820; it was on apoplexy and was founded on the Croonian lectures for 1819.

[37] Brodie, who had delivered the Croonian lectures before the Royal Society in 1810 *On the influence of the brain on the action of the heart* (*Philos. Trans.,* 1811, *101,* 36), repeated LeGallois's experiments in 1813 and publication had been ordered. Brodie, however, had requested that this should be delayed as he wished to undertake further work on the subject. It was first published in Brodie's *Collected works,* London, 1867, *2,* 97.

Benjamin Brodie,[38] thought Wilson Philip! He took up his pen and charged that worthy surgeon with having misrepresented him. "I ask you, sir, as a gentleman," he wrote, "and a man of science to say whether the above mentioned experiment can be regarded as a fair representation of my galvanic experiments." A week later Brodie wrote from 16 Saville Row saying that the only possible difference between the two sets of experiments concerned the mode of application of the galvanism. In the last few days he had therefore repeated the experiments strictly adhering to Wilson Philip's instructions, but he again obtained a contrary result. He was sorry that he could not agree with him; he only wished he could, but he hoped he would not take offence. No doubt some other experimenters would take the matter up—if they thought it to be of sufficient importance—and show one or other of them to be right and the other wrong. He concluded by saying that he declined to enter into any controversial discussion for which he had neither leisure nor inclination.

Wilson Philip, however, pursued Brodie relentlessly, even going to the length of sending an uncle to deliver one of the letters. Would he be good enough to answer five questions? He was sorry to trouble so busy a man but he had no option as his reputation as a scientist was at stake. Would he please be quick as he wished to make the correspondence public and must go to press shortly. He enclosed a further attestation from Hastings and another from Sheppard saying that he had obtained the same results on two small dogs as in rabbits: the dogs had been fed on mutton. Brodie's replies became briefer and briefer but always polite. Wilson Philip hoped to be in London in the autumn and would like to repeat his experiments in Brodie's presence: there *must* be some reason for the divergence in results. Brodie was delighted to hear that Wilson Philip was to be in London in the autumn: he would give him every possible facility for repeating his experiments.

While he was corresponding with Brodie, Wilson Philip was preparing to argue with two other men who had disputed his findings, namely, William Hutchinson[39] and Charles Henry Parry.[40] Wilson Philip's strictures on the latter are rather severe

[38] Wilson Philip would surely have guessed that Brodie was one of the experimentalists because he must have been familiar with the latter's paper *On the influence of the nerves of the eighth pair on the secretions of the stomach* (*Philos. Trans.*, 1814, *104*, 102). He had deduced that division of the vagi brought about suppression of gastric secretion.

[39] *London med. phys. J.*, 1820, 43, Proemium.

[40] Charles Henry Parry (1779-1860), physician at Bath.

when he compared him unfavourably with his father Caleb Parry[41] who, he said, had the modesty of a philosopher and the language of a scholar and gentleman.

On 6 March 1820 Wilson Philip was in Worcester for the baptism of his third son, Charles Henry, at St. Nicholas Church, but later in the summer he shook off the Worcester dust from his fastidious boots, determined to chance his future in London. He magnanimously presented fifty guineas to the Infirmary whose ethical standards had disappointed him. The governors made him a vice-president, while the doctors forgivingly elected him to honorary membership of their medical society. He set up in practice in Hanover Square[42] and soon became one of London's leading physicians.

Meanwhile, Matthew Baillie continued to give him moral support, presenting another of his papers to the Medico-Chirurgical Society of London, this time *On the nature and treatment of the calcareous diatheses.* "The paper was distinguished," wrote one reviewer,[43] "by ingenious reasoning and sound principles of practice."

On 3 November 1820 Sir Henry Halford was presiding over a regular meeting of the Council of the College of Physicians when Wilson Philip appeared before them demanding to be examined for the licentiateship.[44] When asked if he wanted anything further, he replied in the negative. He produced a certificate from Dr. Andrew Duncan, Junior, testifying to the fact that he had taken the degree of Doctor of Physic in the University of Edinburgh. He could not, he said, produce the diploma because he had lost it, but he pledged his honour that he had once possessed one. He was examined *in "parte physiologia"* and told to report a month later for *"parte pathologia."* He was admitted on the 22d of December. Four months later he dedicated his *Treatise on indigestion and its consequences* to the President, Fellows, and Licenciates of the College. As a member he was deeply impressed, he told them, with the advantage which the profession had derived from "the institutions, as well as the science, of that College." The Fellows would learn with concern and perhaps with surprise,

41 Caleb Hillier Parry (1755-1822), author of *Elements of pathology and therapeutics* (1815) and *The nature, cause and varieties of the arterial pulse* (1816).

42 This was probably some time in the late summer, but it is noteworthy that Edward Jenner sent his circular letter of 1821 to "Dr. W. Philip, M.D., of Worcester" (LeFanu, 1951). Munk (1878) says he was living in Hanover Square in 1821.

43 *Med.-chir. Rev.*, 1820, *1*, 607.

44 I am grateful to the Registrar of the Royal College of Physicians of London for allowing me access to the Annals.

that, with the exception of beef, the flesh of old animals is more easily digested than that of young and that the flesh of land animals is gastronomically preferable to that of fish, and that apart from roasting and boiling, the art of cooking is nothing but that of pleasing the palate at the expense of the stomach. The lightest animal foods after mutton, he told them, were chicken, hare, and partridge. But nature produced some peculiarities, for he knew one dyspeptic who could stomach fried bacon but not mutton: perhaps the tastiness of the former encouraged a flow of gastric juice, whereas the blandness of boiled mutton failed to do so. Perhaps this was the rationale of "longings"; at any rate Wilson Philip saw wisdom in the adage of "a little of what you fancy does you good." Butter he regarded as oppressive: perhaps olive oil was better. His greatest gastronomical heresy, however, is contained in the statement that "the refinements of the cook are at variance with the objects of the physician." He was discriminating and sympathetic in the recommendation of exercise to his dyspeptic patients. Horseback riding was excellent treatment for robust dyspeptics but not just after meals. For the others, walking or gardening were good, if one avoided fatigue. The gentlest form of "gestation" was sailing which was good for debility of the bowels as well as of the stomach; and next, the motion of a carriage or a swing, the disadvantage of the former being clearly the risk of catching cold. Night air was bad for dyspeptics and especially London fogs which he attributed to the warm currents of air being set up by innumerable coal fires. He made no mention of atmospheric pollution; in fact, he remarked that the smoke in towns did not appear to be particularly unwholesome.

This book seems to have been well received by the profession[45] and was so rapidly exhausted that a second edition was called for within a year. "Dr. Philip's book has passed the ordeal of criticism with triumph and may now be ranked among the standard productions of the British press," wrote one reviewer.[46]

In London Wilson Philip continued with his experiments, some of which, with the approval of Sir Humphry Davy, President of the Royal Society, were carried out in the Royal Institution in collaboration with Benjamin Brodie with whom he was now reconciled. Indeed, Brodie communicated a paper of Wilson Philip's[47] to the Society, having agreed that the voltaic battery

[45] *Med.-chir. Rev.*, 1821-2, 2, 284.
[46] *Ibid.*, 1821, 2, 862.
[47] *Philos. Trans.*, 1822, *112*, 22.

obviated the effect of dividing the vagi. Wilson Philip's theory concerning the role of the vagus in digestion, however, was challenged by Samuel Broughton, Surgeon of St. George's and St. James's Dispensaries. The Royal Society decided that this was a matter of such fundamental importance that the controversy ought to be settled once and for all and under their auspices. "Even those who were averse to experiments on living animals agreed that it was necessary to set the question at rest by a public repetition of the experiments," he wrote. Benjamin Brodie, because of his experience in experimental physiology, was chosen with two others to repeat these particular experiments which Hastings had made in Worcester. Dr. Andrew Knight, Sir Humphry Davy, and many others were present. The rabbits were first fed on parsley, their vagi were cut, and half of them were "subjected to the galvanic influence." The results were as Brodie had reported before. He next repeated the experiments only severing the nerves below the level where the branches are supplied to the lungs; the same findings were noted. Broughton obtained similar results, but he does not appear to have been so expeditious as Brodie, for his rabbits spluttered with obvious dyspnoea after the pneumogastric nerve had been severed (LeGallois had attributed this dyspnoea to closure of the glottis, congestion of the lungs, and effusions into the bronchi). In spite of the discomfort under which the animals laboured before their despatch, the test parsley was already partly digested. Wilson Philip must have felt foolish, yet it is recorded[48] that he acknowledged in the most candid manner the results of the experiments which were so contrary to what he alleged to be his constant experience. He was now in a quandary, so he wrote again to Hastings. Could there have been a mistake in their experiments? Hastings accordingly repeated them and found that digestion *was* interfered with as a result of section of the vagi. So that there could be no doubt about his observations, he sent his dissected rabbits with the parsley *in situ* to London for Broughton's inspection. They must have presented a malodorous problem in transportation to the postilion of the *Berkeley Hunt*, but doubtless he was resourceful and used to coping with unusual packages. Meanwhile, an assistant surgeon named Cutler in Broughton's former regiment, the Second Life Guards, helped Wilson Philip to carry out further experiments. Hastings prepared an elaborate refutation of Broughton's findings and de-

48 *Edin. med. surg. J.*, 1822, *18*, 301.

fended Wilson Philip's doctrine. "We must decline," wrote the commentator in the *Edinburgh Medical Journal,*[49] "interfering with the respective charges of incompatibility or misrepresentation which these rival experimentalists bring against each other or noticing the zealous reclamations which have since passed between them." Wilson Philip sought a further interview with Broughton and together they worked out a compromise the results of which, as the commentator already referred to wrote, "have at least demonstrated to the world the disinterested and truly philosophic feelings of the parties." The differing results, they agreed, probably depended upon the different ways employed in dividing the nerves, and it was conceded that Wilson Philip was correct in asserting that galvanism tended to restore digestion after division of the pneumogastric nerves "under circumstances in which it is impeded without the galvanism." The controversy with its discordant results did not help to instil in the minds of the profession the importance of physiological research. Such trials offered no encouragement to future researchers. "How could one attribute value to the results of experiments like these entailing as they did so much injury to the animals?" questioned the commentator. He concluded:

> We cannot divine what has tempted Wilson Philip to fix on this unfortunate 8th pair of nerves for all his doctrines on the subject of digestion. . . . Why does he render it to the exclusion and individual agent of so many important phenomena? . . . However we may respect the physiological zeal and indefatigable enthusiasm of Dr. Wilson Philip, still it is impossible to admit the inferences which he has derived from the division of the pneumogastric nerve, such as he has practised it. . . .
>
> . . . . We do not pretend to have smitten to death so many rabbits and dogs as these experimentalists have very laudably done: but this does not prevent us from perceiving that not only have they misled others by their inferences, but they have also deceived themselves by their results.

Wilson Philip pondered on these divergent results for years to come. Eventually, he was led to the conclusion that the differences in results were attributable to differences in technique. Brodie merely divided the nerve, but Hastings had always removed part of it, presumably so that no impulse could conceivably be transmitted across the gap.

Shortly after the performance at the Royal Institution the experiments were repeated in Paris by M. Brechet, Dr. Milne Edwards, and M. Vavasseu, and the results subsequently published.[50]

[49] 1822, *18*, 301.

[50] Presented to the Société Philomatique on 2 Aug. 1823 and published in *Arch. gén. Méd.*, 1823.

That his experiments had been deemed sufficiently important to be repeated in Paris as well as in London was, as the years advanced, a source of increasing gratification to Wilson Philip. He boasted of this achievement on many occasions even though Müller (1835) criticised their validity.

In spite of this controversy and perhaps partly on account of it, his writings continued to be widely read and his views gained general credence, so much so that one commentator wrote of his *Review of some of the general principles of physiology, with the practical results to which they have led:*[51]

> It is a valuable, and, it will be admitted, an unusual feature in the physiological investigations of Dr. Philip and a striking guarantee of their accuracy, that they have led immediately to improvements in the practice of medicine . . . in fine we have no hesitation in expressing our opinion that the various writings, dissections and experiments of Dr. Wilson Philip deservedly place him high in rank among the most illustrious physiologists, pathologists and physicians of the age in which we live.[52]

He was a recipient of a circular letter sent out by Edward Jenner in 1821 (Lefanu, 1951). He had consulted Jenner, it would appear, in the preparation of his treatise on fevers. He seems to have entertained a personal preference for inoculation rather than vaccination because from the accounts he had received, the latter was not only less certain than the former in 'taking,' but it was the more likely to be attended by troublesome ulceration at the site of operation.

He continued to prosper and by 1824 had moved to 18 Cavendish Square (a beautiful house which still stands) where he remained for the rest of his professional life. He had for his neighbour at number 17 Lord Dufferin and at number 19 the Duke of Portland and the Marquis of Titchfield. The other members of the nobility with whose equipages he would have to compete included Viscount Duncannon, the Marquis of Winchester, the Dowager Countess of Pembroke, Viscount Beresford, and Baron Roland, the Swedish Minister. His only medical colleagues were Sir George Tuthill at number 24 and Dr. John Sims at number 37. His patron, Dr. Matthew Baillie, had lived at number 25 until his death in 1823. Without doubt Wilson Philip's interest in gastroenterology and diseases of the liver would serve him well in rectifying the dietetic indiscretions of dyspeptic dowagers, the *bon-viveurs* of the Regency age, and the gouty denizens of West

[51] *J. Sci. Arts*, 1822, *13*, 96, 261; *ibid.*, 1823, *14*, 91.
[52] *Med.-chir. Rev.*, 1822-3, *3*, 596.

Fig. 1. Shieldhall, Renfrewshire. Line drawing reproduced from the *Glasgow Evening Times*, 22 October 1910.

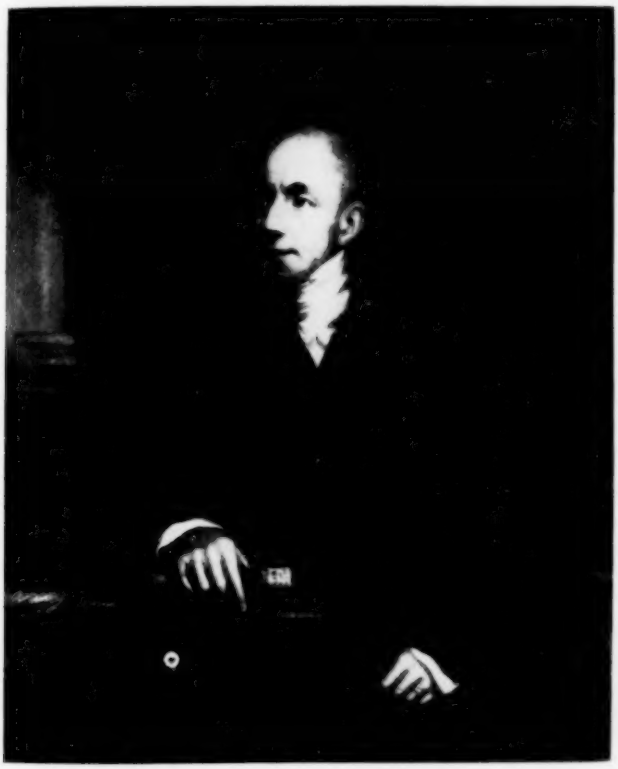

Fig. 2. Dr. Alexander P. Wilson Philip, F.R.C.P., F.R.C.P.E., F.R.S., F.R.S.E. Reproduced from T. J. Pettigrew's *Medical Portrait Gallery*, London, 1839.

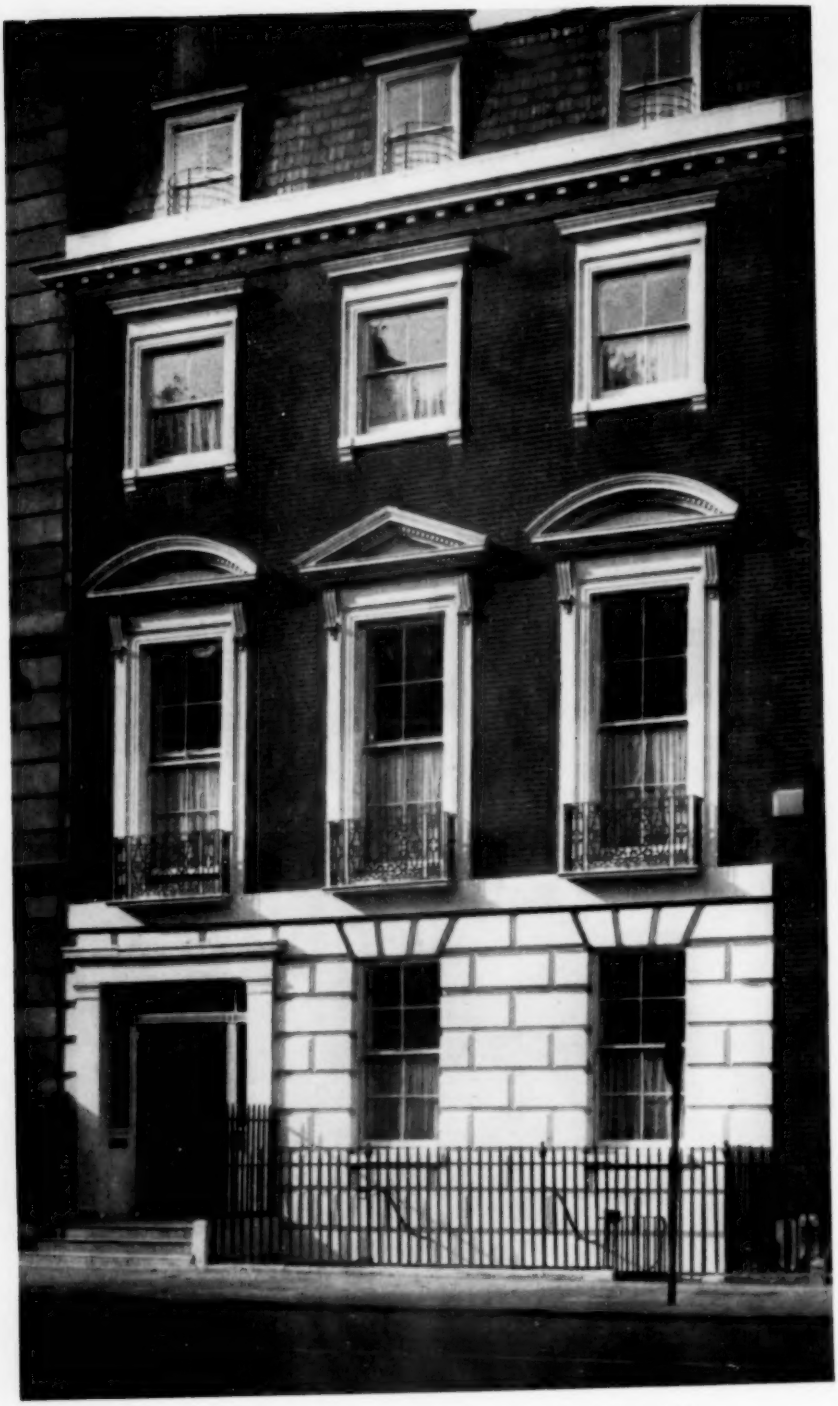

Fig. 3. No. 18, Cavendish Square. London, W.1.

End clubs, while his grandmother's fortune would enable him to provide the appropriately expensive setting for such important and exacting consultations. It was said[53] that with the exception of John Abernethy no man had ever been so successful in attracting public attention to the subject of indigestion.

He was elected a Fellow of the Royal Society on 11 May 1826, his sponsors including Sir Everard Home, Thomas Andrew Knight, William Wollaston, and Benjamin Brodie who clearly had forgiven him for the heated dispute of six years earlier. Altogether, he published thirteen papers in the *Philosophical Transactions,* six in the *Quarterly Journal of Science and Arts,* and ten in the *Transactions of the Medico-Chirurgical Society.* His treatise on *Indigestion and its consequences,* which was first published in 1821 and reached its seventh edition in 1833, was followed in 1827 by a monograph *On the treatment of the more protracted forms of indigestion.* In it he rebuked both Dr. Ayrton Paris, who was another of his sponsors, and Dr. James Johnson[54] for having misunderstood him and misquoted him. Personally, he had always tried to keep his mind unbiased and he would strongly recommend the same maxim to them. He added somewhat offensively: "It is the respectability of the foregoing gentleman alone which has induced me to reply to them; for I find nothing in their works but opinion brought in opposition to the facts I adduce, and as I have neither time nor inclination for controversy I shall not in future be easily induced to reply to any similar observations." The *Medico-Chirurgical Review,* which up to now supported him, began to suspect that Wilson Philip was riding a hobby horse.[55] As a friend and a physician they had respected him, they said, in no ordinary manner and when prostrate on the bed of sickness, they had solicited his aid which was generously afforded: no stronger proof of their estimation of his skill could be well given. But they must not let their private friendship come between them and their duty as reviewers. They then proceeded to deliver an unfavourable judgment on his latest work, alleging that his pathological assumptions were unfounded and his conclusions based on insufficient evidence.

On the 23d of June 1830 he sat in the theatre of the Royal Society awaiting his turn to be called to the rostrum. How proud he must have felt, for the chairman on this occasion was H.R.H.

[53] *Med.-chir. Rev.,* 1827, 7, 362.
[54] James Johnson was editor of the *Medico-Chirurgical Review.*
[55] *Med.-chir. Rev.,* 1827, 7, 362.

the Duke of Sussex who must surely have spent a most tiring and unprofitable afternoon. Wilson Philip was the sixth of the thirteen speakers and he chose for his subject *The nature of the process on which the circulation of the blood depends.*[56]

In 1830 he published *A treatise on the nature and cure of those diseases which precede the change of structure.* It seems to have been intended for the general rather than the professional reader, but it is unlikely that any lay person could have understood it. The writing of such publications, wrote Pettigrew, is questionable, while the one-time friendly *Medico-Chirurgical Review* now boiled with indignation.[57] In the whole course of their lives they had never read such a jumble of absurdity, contradictions, and bad physic. They would have thought that the total failure of an earlier but similar enterprise—as his publisher's shelves could attest—might have moderated Dr. Philip's zeal in the illumination of the public or even checked the overflowing of his philanthropy in rendering them independent of doctors as well as of disease. His election to the Royal Society, they said, had turned his head, for ever since they had done him "the honour" of publishing his lucubrations, the ears of the profession had been stunned with the eternal reiteration of the said honour. It was astounding that he should really set out to educate the public. Did he really think that one person understood a fiftieth part of his 'vital laws'? No, Dr. Philip was neither insane nor knavish, but he was very cunning; he was flattering the public because he had learned that they always relish a little flattery. This last book of his was packed with references to his previous outpourings and this also was an extremely politic device: it was, in fact, one of the most ingenious modes of evading the advertisement duty with which they were acquainted. He had nothing new to offer, but he had devised a technique "of multiplying literary bantlings without any new births, but merely by fresh christenings." It was unworthy of a pen from which so much valuable matter had previously flowed.

Wilson Philip next challenged Dr. William Prout who had delivered the Goulstonian Lectures *On the application of chemical science to explain the phenomenon and cure of diseases.*[58] Wilson Philip entered the controversy[59] on a charitable note: "If I am

[56] *Philos. Trans.*, 1831, *121*, 489.
[57] *Med.-chir. Rev.*, 1830, *13*, 311.
[58] *London med. Gaz.*, 1831, *8*, 257, 321, 385.
[59] *Ibid.*, 1831, *8*, 641, 705, 737, 769, 770, 802, 843; 1831-2, *9*, 38, 69.

right in supposing him in error, it is error that arises from the love of a science which has enabled him to do so much." He criticised Prout for assuming, simply because one could analyse urine, that it would be possible to extend the science of chemical detection to other problems in medicine. Prout had predicted that what the knowledge of anatomy then was to the surgeon in conducting his operation, so would chemistry become to the physician. Wilson Philip accused him of neglecting the laws of animal life. "It is impossible," he wrote, "that any knowledge of chemistry should ever bring us acquainted with a single law of the vital functions." Chemistry and physiology would never ultimately coalesce as Prout had suggested. How could chemistry give the physician any knowledge of even the simplest principles of medical treatment? Could it tell him, for example, that a copious discharge from the bowels would relieve the turgid vessels of the brain? The controversy takes us into some departments of physiology which interest all of us. Prout perhaps had not realised that he would be incurring the displeasure of a gastronomical puritan when he commended the French practice of sealing meat by frying with oil and butter and then simmering for several hours so as to reduce it to a state admirably adapted for digestion. Wilson Philip just could not agree. His patients had all done well on mutton moderately boiled or roasted, a food which Prout had despised.

As the argument proceeded, some hard knocks were given and taken. Wilson Philip clearly could not forgive the personal slight implied when Prout said that there had been no advance in physiology during the last twenty years, for had he not been engaged in his physiological researches for exactly twenty years? He saw in these Goulstonian Lectures an attack on physiology and its self-appointed high-priest, Wilson Philip. It was Prout and not Wilson Philip who had thrown down the gauntlet. Wilson Philip criticised his semasiology: he was very careless about his use of terms, he said, and he ought to know better. Prout in return threatened to dip his pen in gall, accusing him of having a fondness for wordy disputes and for being determined to find fault with everything. The public were being treated by him to "the same thrice-told tale." Wilson Philip in the next round refuses to stoop to the language of Prout and his techniques which he says are but those of the lowest pamphleteer.

This acrimonious correspondence reveals surprising shortcomings in the mind of Wilson Philip, a man who in his earlier days was obviously a thinker. He was, it seems, entirely oblivious to the possibilities of clinical and especially chemical pathology in the practice of medicine: "the changes in fluid," he wrote, "are merely the consequences of some fault in the action of the vital organs." How then can it be of help? To know of the existence of the saccharine principle in urine had not helped the profession to cure diabetes; but *he* had cured a case by prescribing minute doses of mercury ("see my article, etc., etc.") And finally supposing Prout was right and a chemical basis could be ascribed to all vital phenomena, could he "operate on the internal organs and their juices as on an animal separated from the original body which produced it and wholly and conveniently subjected to his art."

About this time G. D. Dermott, Lecturer on anatomy and surgery at the Westminster Dispensary in Gerrard Street, published a series of letters entitled *A discussion on the organic material of the mind, the immateriality of the soul, and the non-identity of the two,* which contained cogent criticisms of Wilson Philip's theories. He had admired the doctor's industry and ingenuity but surely he had drawn wrong deductions from his experiments. He then proceeded to question his views on several matters. He could not agree with him that the nervous and electric fluids were identical.[60] Simply because he had succeeded in transmitting the electric impulse along the nerve it did not signify that the normal nervous impulse was electrical, for surely the nerve in this experiment was only behaving as a conductor in the same way as would any other wet cord. He also criticised him for having claimed to prove that heat was a secretion.

In 1831 Marshall Hall published his *Critical and experimental essay on the circulation of the blood,* reviewing the experiments of LeGallois. Were the results observed the effects of crushing the cord specifically, or were they merely the effect of violence and injury inflicted generally, he asked? He recalled the circumstances of the tragic death of the late William Huskisson, M.P.,[61] which was still very fresh in the minds of the public. The "tremendous vehicle" had passed over the thigh and as a result the action of the heart had failed. The surgeon, he said, had waited in vain

[60] See also Dermott, E. V., *Med. Times,* 1841, 5, 38.
[61] He was killed at the opening of the Liverpool & Manchester Railway.

for a stage of reaction which never came. Marshall Hall clearly recognised shock as a physiological response reaction to trauma. But Wilson Philip rebuked him and also Charles Badham in no uncertain terms: "Let experimentalists in the first place prove that they have made some addition, however slender, to our stock of knowledge; it will then be time enough for them to judge their predecessors," he wrote in a patronising way.[62]

In spite of these and many criticisms, however, Wilson Philip maintained his views and developed new ones; in fact, it was about this time that he first saw the possible implications on treatment of the physiological facts with which he had for so long been acquainted: he now saw and subsequently explained the whole of their bearing on the practical development of the profession. He was fast becoming a therapeutic visionary and it was a source of disappointment to him and, indeed, of bewilderment that the profession had not followed his inspired lead.

In 1832 like many another physician he turned his attention to the subject of cholera. At the request of Mr. Chinnoch, President of the Westminster Medical Society, he set out in monograph form his views on the causes and treatment of the disease. These were communicated in the Hunterian Museum in Windmill Street to the members although not before disapprobation had been expressed at what was considered to be bad precedent since the author was not a member. Wilson Philip then published this article purporting that it had been prepared at the request of the society. The *Lancet* was critical[63] of his motive: he had advertised his pamphlet in the *Times*, they had noted. The members of the society were indignant at "this most unwarrantable use of their name" and the President was obliged temporarily to vacate the chair in order to explain his own conduct and to exculpate Wilson Philip who, he reminded them, was too rich a man to seek lucre in this way. Wilson Philip wrote giving what seemed to be a satisfactory reply, but the *Lancet* was still not content: it was "puffery" they claimed, adding, "the notice sent by our friend, in the neighbourhood of Cavendish Square, would be charged at the Stamp Office as an advertisement." No original ideas were expounded in this work, but he subscribed to the view which held that the disease was contagious. He clearly had no clue as to the aetiology. A proximity to fetid smells seemed to increase the liability to the

[62] *London med. Gaz.*, 1832, *10*, 671.
[63] 1831-2, *1*, 445, 632, 663, 711, 773: also *London med. Gaz.*, 1831-2, *9*, 724.

disease, while it seemed to be a fact that those who worked with spices didn't get it. He once more sang the dietetic praise of mutton, this time specifically at the expense of fish. "At all times one should take care of the body" . . . he said, "for the complicated feelings of civilised life, while they sharpen the mind, enfeeble the body." However, during this year of trial he remembered his friends at Worcester: he sent five guineas to the fund for the relief of cholera. In the following year he was made an honorary member of the Worcester Literary and Scientific Society. In the same year he contributed a paper to the Royal Society *On the relations which exist between the nervous and muscular system*[64] and published the 7th edition of his *Treatise on indigestion,* recording his observations on the process of digestion in upwards of a hundred rabbits. New food, he said, never mixed with the old, but found its way first of all into the centre of the organ. The nearer the food was to the surface of the stomach, the more it was digested. It was least digested in the small curvature. At the pylorus the food was more equally digested, the central parts differing less from those which lay near the surface. His principles of treatment were modelled on these observations.

In 1834 he wrote a monograph on *Sleep and death* which was founded on two independent publications in the *Philosophical Transactions.*[65] What was called death, he said, was simply the cessation of sensorial powers, for the nervous and muscular powers continued for a limited time after death. If respiration ceased, sensorial death followed and it in its turn was followed by death of the nervous and muscular systems. He had watched at many death beds and observed that the order of departure of the vital functions was constant unless the sensorial or nervous system was so impressed as instantly to destroy all the functions. His concern was naturally with the experimental sciences, so he had little to say about the metaphysical aspects of death. The veil which separated these subjects, he said, must ever remain impenetrable, "there being no source of information respecting it but a direct revelation from the great Author of our being, or the instinct he has implanted in our nature, for all knowledge is not acquired." Referring to young creatures he said, "they know what is essential to their condition with an accuracy which sets at defiance all the efforts of human reasoning, for their knowledge is the knowledge

64 *Philos. Trans.*, 1833, *123*, 55.
65 *Ibid.*, 1833, *14*, 823.

of the Creator." He acknowledged his indebtedness to the labours and writings of Magendie, Bell, (1811), Brodie (1812, 1814), and Herbert Mayo (1827), and referred to experiments undertaken earlier for him by William Clift. The critics did not receive this well.[66] His conclusions, they said, were often hasty and the reasoning unconsecutive. He was deserting the true path of experimentation and observation and was losing himself in speculation on the merest assumptions. This and other works of his were so idly theoretical and so utterly devoid of the genuine spirit of scientific research that they would have ignored them had not the name of "Wilson Philip" been attached to them. His besetting sin was presumption which was sometimes carried to ludicrous heights. They concluded:

> In parting with Dr. Philip we would in sincere goodwill remind him that the highest reputation may be trifled away. His own has been well earned and long may it last, but if he has nothing in store better than this treatise on sleep and death we advise him by all means to slumber on his laurels.

At the Comitia of the Royal College of Physicians on 25 June 1834 on the nomination of Sir Henry Halford, the President, he was elected a Fellow and gave his faith. The news was welcomed by the *London Medical Gazette*[67] but ignored by the other journals. He attended thirteen comitias in all, the last on 18 April 1837. Reform within the College and within the profession was at that time exciting much interest, as was also the unsatisfactory state occasioned by the passing of the Poor Law Amendment Act. It would appear that Wilson Philip, while he liked to pick a quarrel, had no real use for medical politics.

In 1835 he delivered the Goulstonian Lectures *On the more obscure diseases of the brain*. This might well be described as "the mixture as before." It is a rambling work full of confused thinking, which found a home in the hospitable columns of the *London Medical Gazette*.[68] Even the kindly John Forbes was disappointed when he reviewed it.[69] It was, he said, but a repetition of that which Dr. Philip had already laid before the public eye so frequently and in such a variety of forms. He continued:

> We cannot commend its style which is diffuse and unconnected. The structure of the sentences is often faulty, abounding in grammatical errors, and obscuring the meaning in a multiplicity of words. . . . We entertain

[66] *Med. Quart. Rev.*, 1835, *3*, 70.
[67] 1834, *14*, 823.
[68] 1835, *16*, 256, 296, 327, 374.
[69] *Brit. For. Med. Rev.*, 1836, *2*, 129.

an unfeigned respect for a physician who has devoted so much of the leisure left after toils and anxieties of a large practice to physiological investigations bearing upon pathology.

The *Medico-Chirurgical Review,*[70] however, was much less kindly:

> Thank God we reviewers are not, like the Fellows of the College, obliged to listen to Dr. Philip's review, especially of his own books . . . if he can produce nothing new, he should allow old and effete matters to rest quietly in their tombs. Their annual disinterment can only bring forth the chlorides of satire to correct their fetor. . . . We have no hesitation in averring that a more disingenuous procedure (not to give it its proper name), was ever ventured upon in puffing pamphlet or advertisement that is here published to the world and that in a Goulstonian Lecture. We defy Dr. Philips, we defy the College, we defy the whole profession, to adduce a more shameless piece of self-exaltation or a more gross insult to the attendant physicians than is here portrayed.

They accused him of having engineered his Fellowship in "the Temple in Pall Mall East." Was it really worth his while to "brush the dust from the feet of the young and unlicked cubs of Oxford and Cambridge in order to sit at the bottom of the bench and with a grateful tear in the eye of the ancient neophyte adore and burn at the shrine of the president?" It was pathetic to see what happened when ambition took the wrong direction: the laudable love of fame had dribbled down into the paltry vanity of personal titles and distinctions. Wilson Philip was furious and complained in the *London Medical Gazette*[71] of the treatment he had received at the hands of the reviewers of the *Medico-Chirurgical Review*. He attempted, as best he could, to vindicate himself of the charge of having "rigged" his fellowship.

The *Lancet* ignored the Gulstonian Lectures, but after their publication several letters appeared in the *London Medical Gazette*. J. W. Earle[72] criticised one of his experiments, and D.J.C. Williams[73] of Half Moon Street took him to task for considering heat to be a secretion resulting from the operation of the nervous energy on arterial blood. To attribute to electricity all the phenomena in which its presence might be detected was about as logical as to attribute to gravitation all the phenomen which ponderable bodies exhibited, wrote Williams. This produced another lengthy correspondence, but throughout Wilson Philip and Williams observed the highest courtesies to each other. Wrote the

[70] 1835, *23*, 515.
[71] 1836, *17*, 55.
[72] 1835, *16*, 449, 789; 1836, *17*, 445.
[73] 1836, *17*, 85, 346.

former,[74] "Had the style of Dr. William's paper been as well directed to the point in discussion as it has been gentlemanlike, our discussion would have been more useful." But Williams evidently knew his man.[75] "I have delayed these observations until after the appearance of Dr. Philip's last letter, lest I should again incur the charge of having misunderstood him."

By 1836 he had completed his "inquiry." In the same year the Royal Society published in concise form a recapitulation of the various papers he had published in the *Philosophical Transactions,* which fact suggests that he still had his supporters in the Society. His thirteenth and last paper in this journal appeared this year: it was a lengthy effusion entitled the *Functions of life.*[76] In the *London Medical Gazette* for 1838[77]—surely a very tolerant and long-suffering journal—Wilson Philip published a series of articles *On the practical inferences which result from late physiological investigations respecting the laws of the vital functions.* Evidently his meaning was not clear to some of his more sympathetic readers, or perhaps it was not clear to himself, for shortly afterwards he sent to the editor[78] a recapitulation of his views to make it clearer to the reader. There were, he said, four distinct sources of power in the human body, all having a direct dependence on each other: (a) the sensorial, (b) the nervous, (c) the muscular, and (d) the living blood. He seems to have had a reasonably clear view as to the relative parts played by the muscular and nervous systems. Voluntary movements were achieved by muscle innervated directly from the brain and spinal marrow, whereas the muscles of involuntary motion received their stimulus through the great chain of ganglias, "each muscle receiving its nervous influence from every part of the brain and spinal cord." He believed secretion depended upon the nervous influence although the source of secretion was the blood. "Calorics," too, he believed to be the result of this influence on the blood.

That knowledge concerning the physiology of the brain had advanced little since the days of Thomas Willis is shown by the simplicity of some of Wilson Philip's beliefs. The division of the brain into cerebrum and cerebellum seemed to him to relate to the sensorial functions since the muscles, both of voluntary and involuntary motion, appeared to bear the same relation to both.

[74] 1836, *17*, 395.
[75] 1836, *17*, 511.
[76] *Philos. Trans.*, 1836, *126*, 343.
[77] *London med. Gaz.*, 1838, *1*, 235.
[78] *London med. Gaz.*, 1838, *1*, 413.

He observed, however, that the sensorial power appeared to be the last to be produced and the first to cease to operate at the end of life.

In 1839 he must have been flattered to find his biography in Pettigrew's *Medical portrait gallery:* indeed, it followed that of Hippocrates. He was described as a distinguished physiologist and physician who "at an advanced period of life" continued to further the objects of his profession with all the ardour and zeal of youth. But the controversial character of some of his views was readily admitted.

> Whatever may be the opinion entertained of the justness of the principles drawn from the experiments—detailed by Dr. Philips—of his industry, his ability for experiments, and of the candour and accuracy of his details there can be no question; and his enquiry must be looked upon as a valuable contribution to medical science.

In the account which followed Wilson Philip's views were set out in the form of 53 aphorisms.

Pettigrew made it the occasion, however, to disagree with Wilson Philip's view on consumption, for the former subscribed to the view which held that the predisposed are born with the seeds of tubercle in their lungs and that no others can become victims of the disease. Wilson Philip in a reply said that it was neither hereditary nor as fatal as was commonly believed. He had seen the disease wholly and permanently banished from families when it had proved fatal to many of its members. He concluded: "When the proper treatment becomes general, the disease which at present annually destroyed so many thousands in this country, will be comparatively rare."

For the next few years we hear little of Wilson Philip's activities. New editions of his books continue, however, to appear; the fourth edition for instance of his *Experimental enquiry into the laws of the vital functions* was published in 1839. He was still trying to justify the views he held during the 1822 controversy on the effect on digestion of section of the pneumogastric nerve. "Mr. Cutler then took them out of the stomach of all of these [rabbits], slit them open, and laid them on the same plate; and Sir Benjamin Brodie was requested to examine them and give his opinion," he wrote at a time when most people had forgotten the incident. Nor could he forget that he had been adjudged correct in some of his surmises. They "acknowledged the accuracy of my statements respected the effect of voltaic electricity in restoring the digestive

powers of the stomach." He made the most of the part played by Brodie who during the intervening years had achieved great fame. Sir Benjamin Brodie had assisted him,[79] . . . "he *did me the honour* of performing the operative part of the experiment." Everyone had come to the conclusion that after all *he* had been right . . . "and Mr. Broughton, who had publicly expressed his doubts on them . . . in the handsomest manner acknowledged their accuracy. . . . I cannot recur to these circumstances without expressing in the strongest terms my sense of the candour and liberality I experienced from all with whom I had any intercourse on this occasion," he wrote.

About the year 1842 he retired from practice and published his swansong entitled *A treatise on protracted indigestion and its consequences.* He had, he said, for years been writing it but had he published it before retirement he would have had to answer to an obvious charge. Now out of practice, he was free to speak his mind. He admitted that various improvements had taken place in the medical profession, but unfortunately knowledge of the nervous system had remained about stationary. He was frankly disappointed with his present-day colleagues who lacked the right physiological approach to the treatment of disease. He wrote:

> The only choice left to me is, either letting the results of the labour of a lifetime on subjects of no common interest be lost to the public at large till a new race of medical men possess their confidence . . . or addressing such a work as the present to the public themselves. Of the present race it is only those who in consultation with me have witnessed the treatment founded on the facts here referred to, who can speak to its effect. Even the modes of regulating even the most essential part of it being still in a great degree unknown to others, although I have done all I could to make them generally known.

The work is a tiresome repetition of the same old theme. He is back again to his Edinburgh days and to the age of Haller and LeGallois and his experiments with Hastings. Had he not laid before Sir Joseph Banks in 1815 an account of his experiments and had they not been publicly repeated in London and Paris? It was true that the results were not published till 1822 but the 42d volume of records of the Society had *specifically* stated that an account of the experiments *had* been laid before Sir Joseph on

[79] Benjamin Brodie, who was eleven years younger than Wilson Philip, received his baronetcy in 1834. He was a Fellow of the Royal Society at the age of 27 and the recipient of the Copley medal only a year later.

January 25th and February 1st and 8th in 1815. The same bees were still buzzing in the same old bonnet. Certainly he was now completely discredited in the eyes of the profession. During the years which had elapsed since his controversy with William Prout he had modified his views in the direction of his former opponent without, however, acknowledging this fact: indeed, he now claimed to be the originator of the idea. The functions of nerves were in essence chemical in nature. He had reason to believe, therefore, having regard for the known simplicity of the works of Nature, that nerves depended for their function on the same agent that operated in the chemical changes of inanimate nature, an inference which appeared to be confirmed by the experiments of Dr. Faraday to whom he had written to ask when this idea had occurred to him that all chemical processes were essentially electrical phenomena. In his reply Faraday (10 November 1841) had said that the first expression of the opinion that chemical and electrical forces were essentially the same was in 1834, but the gradual progress of the idea had been developing in his mind since the beginning of 1833. Wilson Philip wrote in his book:

> Thus, at the distance of five and twenty years have my inferences respecting the nature of the nervous influence derived no trifling support from the labours of this philosopher, so celebrated particularly in the chemical department of science.

Nevertheless, he had not entirely abandoned the vitalistic school of thought and merely compromised his chemical knowledge with his presbyterian beliefs. He wrote in a characteristically muddled paragraph:

> The nervous power, properly so-called, is under the influence of the sensorial power. . . . The muscles of voluntary motion are called into action by the application to them of the nervous power properly so-called, that is as appears from what has been said of voltaic electricity, which is found by direct experiments to be the most powerful of all the stimulants of the muscular power placed through the vital properties of the organs of the nervous powers, immediately under the influence of the sensorial powers.
>
> In this way alone can the sensorial powers influence the external world. . . . The capabilities of the animal are the immediate gift of the Creator; but the stimulants which call its powers into action are always directly or indirectly the same—the agents of the external world.

This tiresome work included thirteen appendices. The reviewer to the *British and Foreign Medical Review*[80] must have sighed as he wrote, " . . . he finds his professional brethren so dull as not

[80] 1843, *15*, 524.

to attend to his lucubrations. The work is disappointing," he wrote, "and a lot of it is meaningless." The other journals ignored it altogether.

The final chapter in the story of Wilson Philip was told by Munk. About the year 1843 or 1844 he suddenly disappeared from Cavendish Square to escape prison and went to live in Boulogne. It was rumored that his investments had proved injudicious and that he had lost the whole of his accumulated fortune. He was believed to have died about 1851 when his name ceased to be on the roll of the Royal College of Physicians.

At Somerset House, however, one learns that he had resided latterly in Dunkirk and that letters of administration were granted to William Grane on behalf of Mary Wilson Philip, his widow, on 2 November 1847 and that the estate totalled £40. Mrs. Philip herself took out a grant on the 27th of March, by which time she had returned to England. The rent and rate books of his house in Cavendish Square provide no evidence of default or hurried departure, for he paid the rent of £500 and the rates of £65.16. 8. up to the end of Michaelmas 1842.[81] His named ceased to appear on the register of the Royal Society in the year ended 30 November 1850, so it looks as if they had been better informed than the College. At Edinburgh we learn that his name ceased to figure on the register of the Royal Society sometime before 1810: he continued always to sign himself F.R.S.E. Because he was not in the Edinburgh locality, his name was not on the list of Fellows of the Royal College of Physicians of Edinburgh who received notices of meetings. It is a tragic story. We can picture this lonely and broken old man, unknown in exile, discredited and forgotten at home, in some wretched lodgings in a foreign port with his devoted wife beside him and only a few of his favourite editions at hand to remind him of better days. He would think back on his life of affluence in London and on his beautiful home which overlooked the trees in the square, remembering how the morning sun poured into those lovely front rooms. He would curse the cruel fate which had robbed him of an easy old age, recalling how insolvency had also overshadowed his boyhood days when they were forced to leave Shieldhall for a family home elsewhere. But misfortune in youth had taught him to work hard and that had brought its own reward. Now he was too old any

[81] Information kindly supplied by Mr. Geoffrey B. Stephens, F.L.P. of St. Marylebone Public Library: he tells me that the rents in the square varied from £125 to £1250.

longer to find solace in work: but he had many publications to his credit and surely they would live after him. In time the world would learn to be grateful to him for his physiological principles.

In 1840 William Makepeace Thackeray published in *Fraser's Magazine* his "Shabby Genteel Story," which concerns George Brandon, a ne'er-do-well, who, while wintering in Margate to escape the dunning of tradesmen goes through a form of marriage—bogus, as it afterwards transpired, with Caroline, the Cinderella-like daughter of the lodging-house keeper. Viscount Clinqbars, the dissolute and consumptive heir of the Earl of Ringwood is the sole witness. In the "Adventures of Philip" published in the *Cornhill Magazine* in 1862, Brandon reappears as George Brand Firmin, the famous West End doctor, husband of the Earl of Ringwood's niece, physician to the Plethoric Hospital and to the Grand Duke of Gröningen and member of that exalted nobleman's personal order of the Black Swan. There is an unexpected and dramatic meeting between Firmin and Caroline, the girl he deserted in Margate, when he is called in to attend his son Philip who is sick with a fever at school. Caroline, now a nurse, is serving him cold tamarind drinks. Dr. Goodenough, the excellent general practitioner who knows Caroline's story, cries out as she swoons to the ground, "You infernal villain! You are the man," and he utters an oath. "Hush! The patient if you please, Dr. Goodenough," replies the great consultant as he turns to observe his son.

One evening at one of his fashionable dinner parties, Dr. Firmin is summoned by electric telegraph to Potzendorff to attend the Grand Duke. He leaves the party at once, never to return. His sudden disappearance, on the eve of a baronetcy, as he afterwards explained to Philip, and the insolvency which has precipitated it, are the talk of the clubs. He turns up, still the incorrigible and reckless speculator, in New York where he devises ingenious ways of borrowing money from the son he has already ruined. He writes to Philip claiming a share in a great and revolutionary discovery in Boston, which turns out to be the use of ether as an anaesthetic (called chloroform in the novel). A sample of Firmin's Anodyne, as he calls it, is sent to Caroline who uses it to great advantage—in a scene surely as exciting as any devised in Hollywood—when she is threatened by the drunken and blackmailing parson who had performed the illegal service of marriage in Margate many years earlier.

The late Dr. J. F. Payne conjectured that it might have been the circumstance of Wilson Philip's disappearance from Cavendish Square that suggested to Thackeray the character of Brand Firmin. It is therefore of some importance to ascertain if Thackeray was acquainted with Wilson Philip and, if so, if there is any resemblance between the doctor in the novel and the doctor in real life. If Wilson Philip was really like Brand Firmin, then he was undoubtedly a scoundrel. Some extracts from the *Adventures of Philip* will serve to tell us something of the doctor in the novel. We read that "Firmin's appearance was so tall and grand, that he looked vastly more noble than a great many noblemen. Six feet, a high manner, a polished forehead, a flashing eye, a snowy shirt frill, a rolling velvet collar, a beautiful hand appearing under a velvet cuff—all these advantages he possessed and used." He was "ever scented and neat, the pattern of handsome propriety," but "this defiant and darkling politeness did not always please the choleric Earl of Ringwood, who was accustomed always to slavish compliance and eager obedience. . . ." "A swaggering fellow, sir," said Ringwood, "but a servile fellow. The way he bowed and flattered was perfectly absurd . . . his house was monstrously expensive." Much of this description would do for Wilson Philip.

His dinners and equipages, it seems, were models in their way. Certainly they earned the approbation of Major Pendennis. But he was also openhanded and generous, subscribing to many charities, while even in exile he held the plate in church bowing "with a melancholy grace to the ladies as they put in their contributions." Arthur Pendennis as a schoolboy always found him polite and gentlemanly. "He passed the bottle to me as if I were a man," he said, and he always left the doctor's house with a substantial "fee" in his pocket. He recalled this pleasing streak of generosity in Dr. Firmin's character when, in later life, he regretfully came to the conclusion that he had been a humbug. Pendennis describes seeing Brand Firmin in the Megatherium Club, a pseudonym sometimes used for the Athenaeum, to which Thackeray was elected to membership in 1851. Wilson Philip, however, was never a member; neither did he belong to Thackeray's other two clubs, the Garrick and the Reform. In the years 1843 and 1844, when it is conjectured Wilson Philip disappeared, Thackeray was for a large part of the time in France and Ireland and in his letters makes no mention of Wilson Philip's debacle.

Finally, we have the suggestion advanced by Professor Gordon Ray that the character of Brand Firmin was modelled on a highly successful and scurrilous barrister named Edwin John James[82] who, in 1861, after being disbarred for malpractice, fled to New York owing his creditors £100,000. Certainly this escapade is more likely to have been fresh in the mind of Thackeray than that of Wilson Philip eighteen years earlier. It was also the year of the passing of the Bankruptcy Act when insolvent professional men were for the first time protected by law. It may be therefore that James's flight and the impending change of legislation would occupy Thackeray's attention. Even if Thackeray had recalled the circumstances of Wilson Philip's flight and made use of it in his story, he would not necessarily have modelled the character of Firmin on that of Wilson Philip. As in dreams, with the rest of us, the novelist in his phantasy delights to make use of the mechanisms of condensation, tranposition, and projection: characters are merged, occupations are altered, and family histories are interchanged.

Was Wilson Philip then merely unfortunate? Certainly we have no evidence to suggest any personal failings which might have necessitated a flight to France. Prior to the Bankruptcy Act of 1861, it was the recognised way for an insolvent professional man to escape perpetual financial persecution, because the future estate of an insolvent remained liable for his debts even after his discharge.

I do not think Wilson Philip was a humbug. Mrs. Robinson's[83] portrait of him as engraved by H. Cook reveals a fashionable man with an assertive air. One can well imagine that he was outspoken, quick-tempered, humourless, obsessional, argumentive, and even conceited, but he does not look like a rogue.

One can regard him as having been a devotee of learning and a worthy and meritorius son of Aesculapius whose early promise unfortunately was not fulfilled. In Winchester, in Worcester, and in London he was too much the lone wolf. Had he been on the

82 It is not known if Thackeray knew James, but in a letter to Thackeray from Charles Dickens, written in 1858, an opinion of James's is quoted with some confidence.

The model for George Brandon is thought by Professor Ray to have been Henry Matthews, a dissipated dandy whom Thackeray knew at Cambridge; he appears also as Bloudell in *Pendennis*. When Thackeray wrote the "Shabby Genteel Story" he almost certainly had no idea how he was to continue it; indeed, until 1852 he seems to have forgotten that he intended to revive the characters and, according to Professor Ray, he did not give the matter serious thought until 1857.

83 A. H. Driver. *Catalogue of engraved portraits in the Royal College of Physicians of London*. London, 1852. The original portrait has not been traced. Note that in his right hand he is holding a copy of his *Laws of the vital functions*.

staff of a teaching hospital and had he been temperamentally able to accept criticism from colleagues possessed of a knowledge of the subject equal to that of his own, he would have pursued further those researches which in their day were esteemed invaluable. As it was, Wilson Philip failed where James McKenzie succeeded Nevertheless, his devotion to physiology, his critical outlook, and his diligence win our admiration: his endeavours deserve to be remembered.

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Early History of Pulmonary Surgery

HORACE HERBSMAN*

TODAY it is commonplace for a surgeon to open a thorax and perform a pneumonectomy, segmental resection, lobectomy, or other extensive procedure which a scant fifty years ago would have seemed sheer wizardry to most of the professional world. The thoracic cavity had long been feared by surgeons. To enter it meant pneumothorax and almost immediate death. The ancient Greeks apparently knew the danger of open pneumothorax. Celsus,[6] in discussing the question of studying anatomy by dissection of living criminals, a practice in vogue in Alexandria in the third century B.C., stated:

> . . . for the belly indeed, which is of less importance, can be laid open with the man still breathing; but as soon as the knife really penetrates to the chest, by cutting through the transverse septum, a sort of membrane which divides the upper from the lower parts, the man loses his life at once.

Intimately associated with the development of pulmonary surgery has been the evolution of the modern concepts of respiration and open pneumothorax. Any type of thoracic surgery had long been dreaded but as the clinical observations of hundreds of years began to be explained by newer physiological findings, surgeons were more wont to enter the pleural cavity.

Acute empyema, a disease quite uncommon today, was undoubtedly the earliest pulmonary affliction to be treated surgically with any degree of success. Moreover, the basic physiological principles uncovered by the study of this disease, as recently as the early years of this century, have greatly contributed to our present attainments in thoracic surgery.

Hippocrates[25] had correctly regarded acute empyema as an abscess of the pleural cavity which should be treated by drainage. Adams, in his translation of the Works of Hippocrates, notes the following:

> When the collection protrudes externally, he directs that an opening should be made in it; but if not, he directs the patient to be shaken by the shoulders, when the sound of the fluid within will be heard. When the side in which the greater collection is situated has been ascertained, he recommends us to cut down to the third rib from the last, and then make a

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perforation with a trocar, so as to give vent to a small portion of the fluid; the opening is then to be filled with a tent, and the remainder evacuated after twelve days.

Mortality was necessarily great and with the invention of the aspirating syringe several centuries later, the method fell into disrepute. Too early incision, with disastrous pneumothorax resulting, also helped to make aspiration the preferred treatment. Hamilton Roe,[48] before the Royal Medical and Chirurgical Society of London in 1844, advocated aspiration as the best therapy, citing nine cases of purulent effusion treated by this method, with eight recoveries and one death. Henry Bowditch[3] of Boston in 1852 popularized the practice of thoracentesis in the United States, and it became the customary treatment not only in empyema, but in all pleural effusions. Continuous closed drainage achieved some usefulness, although its greatest value was in later years when it formed the basis for continuous underwater chest drainage following thoracotomy.

While acute empyema was being treated by some method of closed drainage, a more radical approach began to be attempted in cases of chronic empyema, the fibrin-lined cavities resulting from failure to cure the acute disease. The chronicity of the process in these cases produced a fairly rigid mediastinum and permitted performance of an open pneumothorax without disastrous results. The technique of thoracoplasty was set forth in detail by Estlander[13] of Helsingfors in 1879. He suggested removing portions of the overlying ribs, permitting the soft parts to collapse into the cavity, a procedure which later became adopted for the treatment of pulmonary tuberculosis. But for chronic empyema this was inadequate since the overlying parietal pleura was so greatly thickened that collapse of the chest wall did not occur unless it, too, was removed. Schede[50] of Hamburg devised an operation in 1890 in which the thickened pleura was also removed.

Both the Estlander and Schede procedures were considerably mutilating and therefore, in 1893, Fowler[15] of New York and, in 1894, Delorme[10] of France, independently developed a less extensive but highly successful procedure. They actually removed the rigid coating of the visceral pleura from the lung, permitting it to expand enough to help in the obliteration of the cavity. Several modifications of this decortication procedure have since been attempted, such as that of Ransohoff[45] of Cincinnati, who advocated gridiron incisions in the pleura overlying the lung when decortication was not feasible.

The benignity of open pneumothorax in these cases, accentuated by the severity and frequent persistence of chronic empyema despite extensive operative procedures, led to a similar radical approach in acute empyema, intending to arrest the process early and prevent the development of a chronic cavity. Actually, this merely represented a return to the original teaching of Hippocrates and by the early part of the twentieth century the trend was toward prompt rib resection with open drainage for most cases of acute empyema. Thus Stephen Paget[42] in his classic textbook of 1896, *The Surgery of the Chest,* noted that "the old routine treatment by repeated punctures or aspirations has, happily, gone forever."

But the wheel had not yet come full circle. During the winter of 1917 to 1918 an influenza epidemic ravaged American military camps, bringing with it a type of empyema for which open drainage failed disastrously. Mortality averaged 30.2 per cent and in one camp it was as high as 84 per cent.[12]

The U. S. Government appointed an Empyema Commission to study the situation and out of its work came not only the answer to the pressing problem at the time but, perhaps more important, a full evaluation of the basic physiological principles involved in open pneumothorax.

The Commission noted that the Army empyema cases were not due to the usual type of pneumococcal pneumonia but instead were of streptococcal origin. Pneumococcal pneumonia is usually in a state of resolution when significant amounts of pus have formed in the pleural cavity. Streptococcal pneumonia, however, is very often still active when pleural exudate appears and, moreover, the pus is usually thin and watery. The Commission pointed out that mediastinal fixation was important and had not yet occurred when the latter cases were being subjected to open drainage. Consequently, in place of early operation, a method of repeated aspiration during the pneumonic phase was adopted, with a deferred drainage operation. At Camp Lee, Virginia, mortality dropped dramatically to 4.3 per cent.

Considerable skepticism arose concerning the findings of the Commission and two of its members, Major Evarts A. Graham and Captain Richard D. Bell, were permitted to extend their work and perform experimental studies on pneumothorax and empyema at Johns Hopkins Medical School. Their published findings constitute a classic in the field of pulmonary physiology.[17] In addition to confirming the importance of the mediastinum in open

pneumothorax, they emphasized the significance of the vital capacity in the ability to tolerate an opening in the pleura.

The prevalent conceptions of pneumothorax are erroneous in that they are based on the assumption that when an opening is made into the chest one lung is collapsed and the other maintains respiration. This assumption implies that the mediastinum constitutes a rigid partition between the two pleural cavities. On the contrary the mediastinum is so mobile that any increase of pressure in one pleural cavity pushes it over into the opposite one so that both lungs are compressed practically equally. No such condition is possible, therefore, of collapse of one lung and maintenance of respiration with the other in a chest with a normal mediastinum.

If, on the other hand, the mediastinum has been made rigid by induration as a result of long-standing inflammation, or if it has become fixed by adhesions, then a pleural opening on one side will not produce the same pressure changes in both pleural cavities.

The maximum opening into a pleural cavity compatible with life depends upon a definite relationship which exists between the amount of air entering the lungs and the amount which enters the pleural opening. . . . Whenever the amount of air taken into the lungs is limited by the presence of an active pneumonia, with plugging of both air channels and alveoli, whenever there is an excessive demand for air, or whenever there is sufficient weakening of the respiratory muscles to impair compensation, the size of a pleural opening compatible with life becomes smaller; and if any or all of the above factors are present in sufficient intensity, even a very small opening into the pleural cavity will produce death from asphyxia.

Until the latter portion of the nineteenth century, empyema had remained the only pulmonary disease for which elective surgery was attempted. Nevertheless, scattered throughout the literature of the Middle Ages and even earlier are reports of occasional lung surgery necessitated by trauma. Guy de Chauliac,[7] in 1519, described a vivid case of hernia of a lung treated by Roland of Parma (Figs. 1 and 2).

Called to a citizen of Bologna on the sixth day after his wound, I found a portion of the lung issued between two ribs; the afflux of the spirits and humours had determined such a swelling of the part, that it was not possible to reduce it. The compression exercised by the ribs retained its nutriment from it, and it was so mortified that worms had been developed in it. They had brought together the most skillful chirurgions of Bologna, who judging the death of the patient to be inevitable, had abandoned him. But I, yielding to his prayers and to those of his parents and his friends, and having attained the leave of the Bishop, the master, and the man himself, I yielded to the solicitations of about thirty of my pupils, and made an incision through the skin, the breadth of my little fingernail away from the wound, all round it. Then with a cutting instrument, I removed all the portion of the lung level with my incision. The wound resulting from this resection was closed by the blood issuing from my incision, and was dressed frequently with the red powder and other adjuvants. By the grace of God it cicatrised, and recovery took place.

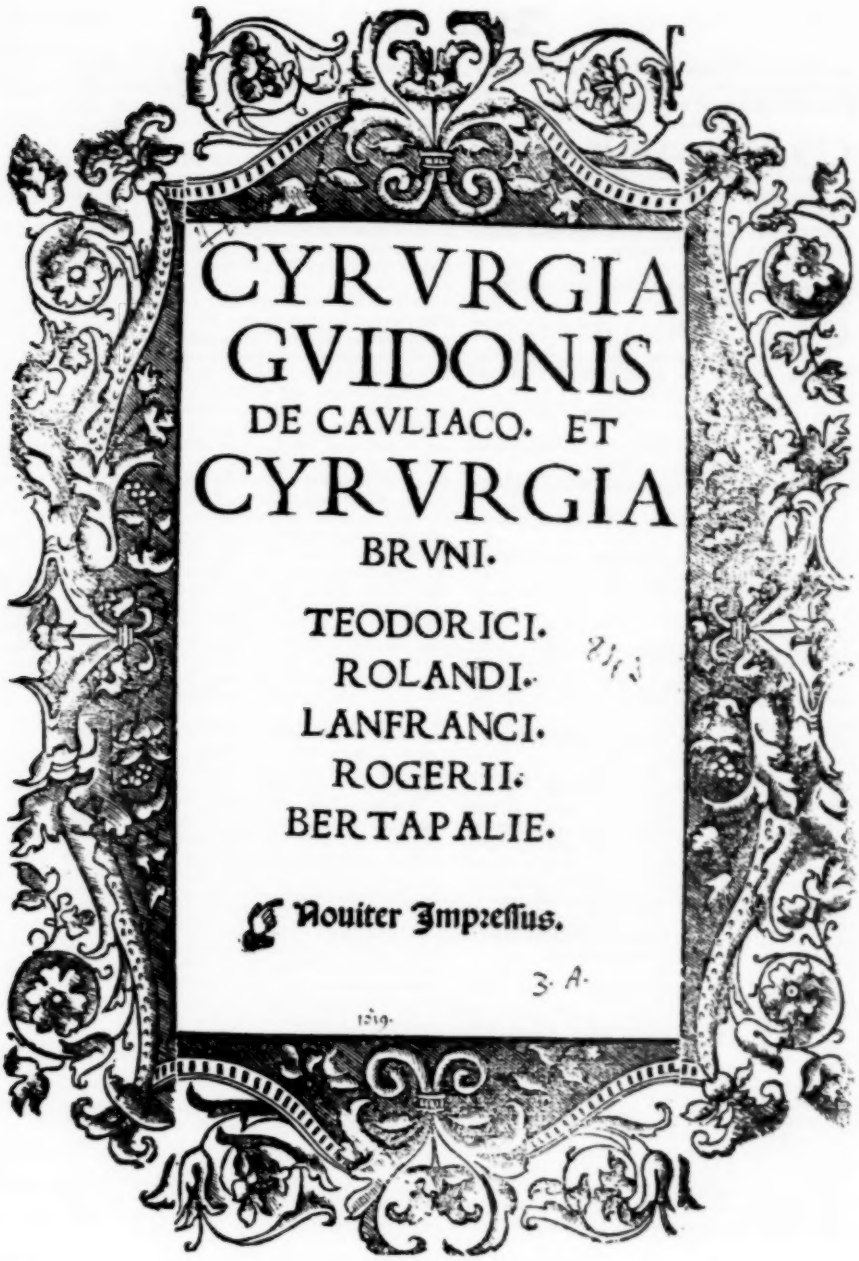

FIG. 1. Title-page of 1519 edition of Guy de Chauliac's *Cyrurgia*. (Courtesy of The National Library of Medicine.)

cere ꝓfucuit: deinde apoſtolicon vel aliquod ſimile emplaſtri ſuperponatur.

¶ De vulneribus ventris ſine vulnere inteſtinorum.
Capitulum. xxi.

SI fiat vulnus in aliqua parte corporis vſq; ad inguem ⁊ inteſtina non ſint leſa, coden modo cura eſt adhibenda vt ditimus in cura thoracis ⁊ pectoris ſine ferrum lateat ſiue non in ferius: excepto q̄ non debet in rotundum: ſed in longuꝫ incidi quando ferrum latet interius.

¶ De vulnere cordis pulmonis. diafragmatis. ſtomachi ⁊ epatis. Cap. xxij.

SI quis vulneratus fuerit in corde. epate. pulmone. ſtomacho. diafragmate cum noſtre cure non committimus. horum enim vulnera bis ſignis cognoſcimus.

¶ Si enim vulnus in corde fuerit. ſanguis inde exit niger.

¶ Si vero ſanguis ſpumoſus fuerit vel exierit abundanter de pulmone eſt: ⁊ anbelitus mutatur.

¶ Si in diafragmate magnus, ⁊ ſpiſſus erit anbelitus: ⁊ cito moritur.

¶ Si in epate manifeſtum erit: per leſionem operationum: que in eo fiunt.

¶ Si in ſtomacho cibus per plagam egreditur: quoruꝫ omnium vulnera mortalia iudicamus. Ne noſtro iudicio videantur perire ab huiuſmodi cura potius deſiſtamus.

¶ De vulnere ſplenis. Cap. xxiij.

SPlen ſeruile membrum eſt. vnde inciſiones: ⁊ alia de facili paritur: quare ſi vulnus habuerit precedentium ⁊ ſubſequentiū cura ſecure: ⁊ cito liberabitur.

¶ De vulnere in regione epatis cum exitu eiuſdem.
Capitulum. xxiij.

COntigit quādoq; epar per vulnus ſtrictum exire: timentes leſionem vulnus non audemus elargare: cui ſic ſubueniemus. ¶ Patiens ſupinus iaceat ⁊ bene diſtentus. medicus vero a ſuperiori parte manibus ex tranſuerſo cutim accipiat. diſcipulus vero ab inferiori parte eodem modo faciat: ⁊ ita talis medicus ⁊ diſcipulus ſe tenendo cutim ad ſuperiora totum corpus feſtinanter eleuēt: vt patiens ſubita aeris attractione quadam propitione ipm epar ad propium locum redire compellat.

¶ De vulnus pulmonis ſanari poteſt. Cap. xxv.

CLamauerunt ⁊ clamāt medici vulneratum pulmonem curari nō poſſe. Horum enim continuum cum humoroſita te aſſidua curationi vulneris afferunt obuiare: quod quidem verum eſt vbi vulneris ſuperfluitas ſufficienter expelli non poteſt. aliter enim falſum eſt quod dicunt. Nā ſi cum extrinſeco vulnere ipſum vulnerari contingat: ſic conuenienter pulmo mundatur ⁊ optime conſolidatur. Quod quidem ego Rolandus Parmenſis gratia fauente diuina eſperim. ſam operando. vocatus enim ad quēdam Bononiēſem quinto vel ſexto die vulnerationis inuenī pulmonis partem, exiuiſſe exterius inter coſtas: ⁊ nimio ſpirituum ⁊ humorum concurſu facta fuerat tanta pars illius inflatio q̄ intromitti nequibat. coſtarum etenim conſtrictione nutrimentum retinebatur interius. vnde exteriora mortificata fuerant: ⁊ vermes etiam intra ſe contuchant. conuocati ergo peritiores cyrurgici Bononienſes cum p̄o mortuo reliquerunt. Ego autem ipſius ⁊ amicorum ſuorum precibus condeſcendēs: ⁊ ab epiſcopo vrbis impetrata: ⁊ tam ab ipſo q̄m a domino ſuo: ⁊ illorum amicorum ſecuritate obtenta. xxx. fere rogantibus atq; aſtantibus cutim circa vulnus p̄o ſtitudine vnguis minoris digiti circuncidi. deinde prope circunciſionem totum exteriorem abſcidi pulmonem. Loco igitur abſciſionis ſanguine circuncīſe cutis conſortato: ⁊ tam pulcre rubeo ⁊ alijs adiutorijs frequenter procurato conſolidatum eſt dei gratia vulnus: ⁊ ſanatus eſt. ille poſt multum vero temporis cuꝫ domino ſuo Berardo Rolandini Jeroſolymam adiit. ⁊ inde iam dudum regreſſus ſanus atq; incolumis viuit. ¶ Si autem queſieris qualiter egiſſem ſi a principio vocatus fuiſſem, reſpondeo q̄ elargato vulnere caute cum cuneo intromiſſo: ⁊ poſtea erecto ſegregando coſtas ad inuicem: pulmonem p̄uio calefactum cum gallo vel gallina ſciſſa a tergo intromiſiſſem: ⁊ vulnus tenuiſſem apertum vſq; ad mundificationem perfectam. ¶ Sed obiicies: nonne melius erat vulnus tantummodo elargari ⁊ conſortari nutrimento: ⁊ ab intrinſeco veniente: ⁊ ſic tandem ſine abciſſione intromitti? Non; quia mortificato exterius membro cum ſua natura non fieret nutrimenti attractio: nec per tertiam digeſtionem cuis incorporatio.

¶ Amplius ſi interrogaueris qualiter homo ille ſine pulmone viuere poſſit. Dico q̄ particula que remanſit interius augumentata eſt ab ipſa natura totius membri accipiens nutrimentum. vel forſitan ipſa natura pennulas alias concreauit. quod in membro molli facile fit: ⁊ propinquitate caloris cordis: cui neceſſarium erat membrum.

*Regiā ri poteſt pticula pul. abſciſſa.

¶ De vulneribus inteſtinorum ⁊ ſi foras exierint qualiter ad propium locum reducantur. Cap. xxvj.

SI inteſtina per aliquod vulnus factum in corpore foras exierint: ⁊ ex obliquo: vel per longū inciſiū fuerit: ita q̄ maior pars inteſtini ſana remaneat: ſic ſubuenimus. Primo ſi inteſtina per aliqua interualla infrigidata fuerint: aliquod animal viuum per medium ſcindatur: ⁊ ſuper inteſtina ponatur: ⁊ tandiu dimittatur ibi: quouſq; inteſtina calefiant ⁊ calore naturali inueniantur: atq; imolleſcant. ¶ Iterum preparetur canellus de ſambuco ad modum vulneris inteſtini: ita q̄ ſuperet longitudine ſua longitudinem vulneris ex vtroq; latere vncia vnā. cancillus quoq; valde ſubtiliter: ⁊ per vulnus inteſtini intromittatur: ⁊ ſubtiliſſima acu ⁊ filo de ſirico ſuatur. cancillus quoq; ita locetur: vt ſuperfluitas inteſtini per ipſum valeat tranſire ⁊ ſecure vulneri impedimentum non exhibeat. Quo facto ſpongia in aqua calida infuſa ⁊ bene ablata: fordes ex inteſtinis ſuauiter abſtergantur. quibus bene purificatis per vulnus: per quod exierint in ventres intromittantur. ⁊ tunc patiens ſupra tabulam poſitus concutiatur: vt etiam primo ſcemate collocentur inteſtina: ⁊ ſi vulnus non fuerit tantum q̄ poſſint intromitti amplectur. Quibus intromiſſis tantum dimittatur vulnus apertum quouſque videris inteſtinum conglutinatum. vnde ſupra futuram inteſtini omi: oie pulnus rubeus ſuperponatur. poſtquā vero inteſtinum fuerit conſo-

FIG. 2. Page from Guy de Chauliac's *Cyrurgia* describing an early case of pulmonary surgery.

Similar cases were recorded by Forde[14] (1836), Hale[23] (1855), Grinnell[21] (1878), and Richards[46] (1880). Surgical drainage was extended to lung abscesses and occasional bronchiectatic and tuberculous cavities, but until 1880 this remained the extent of pulmonary surgery.

In 1881 there appeared almost simultaneously the results of experimental work by Gluck,[16] Block,[2] Schmid,[51] and Marcus,[33] which demonstrated that animals could survive a pneumonectomy or lobectomy. Gluck showed that rabbits and dogs could survive a complete pneumonectomy. He removed the lung after tying a mass ligature around the hilus. In those animals that survived, the bronchial stump was found to be well healed. Block, having trained in the methods of Lister, employed strict antiseptic techniques in performing his pneumonectomies. Nevertheless, most of his animals died of "hemorrhagic fibrinous pleuritis." However, he was convinced of the applicability of the operation to man and, as a result, was soon thereafter beset by tragedy.[43] He chose to attempt to remove both apices of a young lady relative who supposedly had apical tuberculosis. She died shortly after surgery and, confronted with a judicial inquiry in which it was claimed her lungs were not affected, Block committed suicide.

Schmid reported the resection of the apex of the lung, using a temporary ligature to arrest hemorrhage during the operation. He resected the fourth and fifth ribs, drew the apex of the lung into the wound, and controlled it with a constricting ligature. After amputating the apex he tied off the cut surface, removed the constricting ligature, and closed the wound. He, however, did not use antiseptic techniques and of eight animals, only three recovered.

In 1882, Biondi[1] reported a large series of experiments with encouraging results. He performed pneumonectomies on a total of 57 animals with 30 surviving.

With these encouraging results, it was not surprising that sporadic reports of pneumonectomy or lobectomy should begin to appear. Most were unsuccessful. Krönlein,[26] a Swiss surgeon, in 1884 resected a portion of a lobe which was invaded by a rib sarcoma and the patient recovered. Lowson[31] (1893) successfully resected the apex of a lung for tuberculosis. In April, 1895, Macewen[32] removed a tuberculous left lung which was almost completely destroyed by disease. The lung, necrotic and filled with considerable pus, was removed in fragments. After a subsequent

thoracoplasty the patient recovered satisfactorily. Kümmel[27] (1910) reported resection of an entire lung for carcinoma in a 48-year-old man. The patient died six days postoperatively of sepsis arising from necrosis of the bronchial stump.

The scope of thoracic surgery was markedly limited as the twentieth century began. Heuer,[24] reviewing the literature from 1880 to 1920, found reports of nine resections or lobectomies for tuberculosis, 14 for tumors of the lung or of the thoracic wall invading the lung, and 56 for bronchiectasis. Mortality was high, and all of the procedures depended on a fixed mediastinum or lung so that open pneumothorax could be accomplished. Often two-or three-stage operations were necessary to achieve this by first inducing adhesions of the lung to the parietal pleura. This was also necessary to help limit infection, for upon entering the pleural cavity the lung falls away, opening a wide avenue for the spread of infection.

It became apparent that successfully to open the pleura during an operation where the structures were not fixed, a method would have to be devised either to increase intrapulmonary above atmospheric pressure or decrease intrapleural pressure to the same level that existed before opening the pleura. Thus in 1896 Quénu and Longuet wrote:[44]

> We have realized that the essential condition of success in a pulmonary or intra-pleural operation (granted the supposed absence of pleural adhesions), is to maintain a difference of pressure between the intra-alveolar air and the surrounding air, the effect of pneumothorax being to make the intra-thoracic and extra-thoracic pressure uniform. Two means come to mind: to lower the extra-thoracic pressure, the intra-pulmonary tension remaining the same, making it necessary for the surgeon to operate in a relative vacuum, or to increase the intra-bronchial pressure.

Actually, the fact was known to Vesalius who, in 1543, in the last chapter of the *Fabrica* entitled "Some Observations in the Dissection of Living Animals," demonstrated the results of opening the chest of an animal:[57]

> But so that life may in some measure be restored to the animal, you must attempt an opening in the trunk of the trachea and pass into it a tube of rush or reed, and you must blow into this so that the lung may expand and the animal draw breath after a fashion; for at a light breath the lung in this living animal will swell to the size of the cavity of the thorax and the heart take strength afresh, and exhibit a variety of motions.

Here was the key to the future of thoracic surgery. But it was to lie dormant for 350 years.

The first major attempt to cope with open pneumothorax during surgery was made in 1904 by Ferdinand Sauerbruch,[49] assistant in Mikulicz's clinic in Breslau. He utilized the principle of decreasing the pressure on the open pleura to the level that existed before opening the thorax, thus maintaining the physiological difference between intrabronchial and intrapleural pressures. To produce this he placed the patient in a large pneumatic cabinet, allowing the patient's head to extend outside of the cabinet and be exposed to the atmospheric pressure. The surgeon and staff remained inside the cabinet, where a negative pressure of 7 millimeters mercury was maintained, and performed the operation. The cabinet walls were of strong sheet-iron and one double door provided entry without interfering with the constancy of pressure. The reduction of pressure was effected by an electrically driven suction pump. Telephonic communication was possible between the anesthetist and the occupants of the chamber.

Sauerbruch made several modifications on his cabinet in the next few years, and in 1909 Willy Meyer[37] of the German Hospital of New York (now Lenox Hill Hospital) installed a negative pressure apparatus which was the only one in existence in the United States. Until a few years ago, it was still preserved at that hospital although newer methods had long since relegated it to antiquity. One of the modifications which Meyer introduced was a positive pressure cabinet in the negative pressure chamber. This smaller cabinet contained the head of the patient and was large enough to accommodate as well the anesthetist, an assistant, and the anesthesia apparatus. The air pressure within the positive chamber could be raised to 20-25 millimeters mercury. By this arrangement double assurance was afforded that the pneumothorax could be controlled. It became evident, however, that CO_2 would accumulate and the operation had to be frequently interrupted to allow the lungs to collapse and be cleared of CO_2. The expense, difficulty of transport, and overheating of the inside of the cabinet made the pressure chambers impractical for extensive use, and other methods were sought.

The principle of increasing the intrabronchial pressure to prevent collapse of the lung was next applied to thoracic surgery. Actually, it had been attempted several years earlier but had gained no universal acceptance. Tuffier and Hallion[56] in 1896 devised an apparatus for insufflating the lung through an intralaryngeal tube attached to a bellows. The importance of their

work was wisely appreciated as early as 1900 when Rudolph Matas wrote:[34]

> My personal experience in thoracic cases and the evidence I had gathered in the course of animal experimentation had long ago impressed me with the importance of acute pneumothorax as one of the most formidable bars to the successful issue of intrathoracic operations and kept me alert for all practical suggestions to counteract it. But it was not until 1897, when I read an account of Tuffier and Hallion's experiments, that I realized that thoracic surgery was on the eve of a revolutionary innovation.

It was Matas who later replaced the bellows by a pump and suggested several modifications of the original apparatus.[35] The positive and negative pressure chambers passed into history but left as descendents the respirators used today in respiratory paralysis. The insufflation apparatuses of Tuffier, Matas, and others were a beginning, but it remained for Meltzer and Auer[36] in 1909 successfully to develop intratracheal insufflation, the direct forerunner of modern endotracheal closed anesthesia.

The method devised by Meltzer and Auer depended upon the insufflation of air or oxygen in a continuous stream under low pressure (15-20 mm of mercury), combined with varying concentrations of ether vapor, through a flexible silk woven catheter inserted through the mouth and larynx into the trachea, almost to the bifurication. The lungs were kept in a state of normal distention, and the excess of expiratory air escaped through the trachea around the catheter. A foot-bellows was used to blow in the air at a constant rate. Actually, the "insufflation" was positive pressure since the presence of the catheter resulted in some resistance to the air passing out of the trachea around the tube. Therefore, the lungs had to be collapsed from time to time to clear them of CO_2.

Meltzer and Auer and Alexis Carrel conducted many experiments on animals, demonstrating the usefulness of intratracheal insufflation. Charles Elsberg of New York City recognized the great potentialities of the method and constructed a very practical and easily transportable modification, replacing the bellows by a motor-driven blower. He also altered the technique by introducing the tube into the trachea through a laryngoscope under direct vision.

An opportunity arose late in 1909 for Elsberg to test the apparatus on a human, and the following is a published account of one of the most unique experiments in thoracic surgery:[11]

On December 9, 1909, a young woman of thirty-four was admitted to Mt. Sinai Hospital with advanced symptoms of myasthenia gravis. On the 25th, the patient stopped breathing for several minutes but respiratory movements returned again. During the following night, respirations suddenly ceased, the patient became deeply cyanosed, and asphyxia and death seemed imminent.

The insufflation apparatus had been kept in readiness at the patient's bedside. When I reached her, she was unconscious, very deeply cyanosed, pulseless. Without trouble, the mouth was opened, the epiglottis drawn forward with the index finger of the left hand, and the tube passed through the glottis down into the trachea. A mixture of air and oxygen was then passed into the trachea under a pressure of 20 mm.

Within one minute, the appearance of the patient was completely changed; the face became pink, all cyanosis disappeared, the pulse could be felt at the wrist. The pulse rate was at first very slow—32 to the minute—but it soon became of good quality and more rapid—110 to 120.

For about five hours the intra-tracheal insufflation was continued; during this time the patient did not make a single respiratory movement. Her color, however, remained good, and her pulse of good quality. Finally the insufflation was stopped as the condition was a hopeless one.

All that remained was for the method to be used clinically in an intrathoracic operation on a human being. In February, 1910, a 55-year-old man was admitted to the Mt. Sinai Hospital with a thirteen-month history of an abscess of the middle lobe of the right lung. The patient, although his general nutrition and color were good, had been expectorating foul, greenish pus, and the abscess had shown no signs of improving. The possibility of a radical cure was suggested to Howard Lilienthal by the excellent results obtained in animal experiments with the Elsberg apparatus, and operation was planned.[28]

On 14 February 1910, the first attempt to pass the intratracheal tube was unsuccessful because of an irritable larynx, and consequently only a resection of the eighth and ninth ribs was performed without opening the pleura. On 20 February 1910, the larynx was first anesthetized with cocaine and the intratracheal tube passed successfully by Elsberg. The chest was opened through the previous area of resected ribs, "under pressure of 15 millimeters of mercury. It was at once seen that the lung distended to about two-thirds its capacity, mottled and rosy pink in color. Complete pulmonary distention could be secured by raising the pressure to 25 millimeters. The lung was collapsed and distended several times as to observe the working of the apparatus." No cavity or pus could be found and the pleura and wound were closed, the pressure being raised to 30 millimeters while this

was done. No drainage tube was used but saline compresses were placed over the wound to prevent leakage of air. "The patient was then sent to the ward. Ten minutes later he had entirely recovered from his anesthesia so that he held conversation with those about him."

> His recovery was quite uneventful and much to my surprise and delight his expectoration ceased, perhaps because the comparatively large rib resection permitted the cavity to become obliterated. . . . I consider that this was a most severe test of the Meltzer method of anesthesia because of the foul and septic condition of the discharge from the pulmonary cavity. In spite of this there was no pneumonia and not even a bronchitis.

Following the successful use of intratracheal anesthesia, it was expected that more extensive pulmonary surgery would be attempted. But such was not the case. Skepticism concerning this method of anesthesia persisted for several years. Willy Meyer[38] called it the "blowpipe method" and said it was a backward step of fifteen years. It remained for the United States Empyema Commission in 1918 to describe fully and accurately the physiology of open pneumothorax, including the phenomenon of mediastinal flutter. This gave scientific credence to intratracheal anesthesia and it became widely accepted.

Thoracic surgery still remained limited to lung abscess, bronchiectasis, and empyema, but the scope of surgery in these diseases was expanded. Tuberculosis was treated by collapse therapy or thoracoplasty with rare attempts at resection. In 1916 Lilienthal[29] published his first report on lobectomy for bronchiectasis containing six cases, of which four were cured and two died. His first lobectomy was performed in February, 1914, on a 3½-year-old male who had a chronic gangrenous, diffuse bronchiectasis limited to the right lower lobe following aspiration of some partly masticated nuts. Anesthesia was given by the intrapharyngeal method and lobectomy was performed through a seventh intercostal incision without dividing a rib. There was complete recovery, and the patient remained well.

In 1922 Lilienthal[30] published further results with bronchiectasis. Up to that time only 30 cases of partial or complete lung resection for bronchiectasis had been reported in the literature. He added 31 new cases, but with what would now be considered an appalling mortality rate. Surgical removal of a single pulmonary lobe for chronic pus formation had a 42 per cent mortality. When more than one lobe was involved, as in ten cases,

the mortality was 70 per cent. But his outlook was optimistic, much more so than that of Sauerbruch and other contemporaries. He maintained that . . . "Considering the present stage of this newly developed side of thoracic surgery, any percentage of cures over forty per cent may be considered excellent."

He also believed that time was one of the most important factors in the operation. "I have a feeling," he said, "which is more than a mere impression that any longer than forty-five minutes used in performing lobectomy means the almost certain loss of the patient." Therefore, he used a two-stage procedure whenever there was the slightest doubt about the patient's condition. In the first stage he induced adhesions of the parietal to the visceral pleura by rubbing the pleural surfaces with gauze or packing the pleural cavity with iodoform gauze, and then closing the chest with the lungs under positive pressure. Lobectomy was performed at a subsequent operation, when differential pressure was often not necessary since the lung was now fixed. The pedicle in that day was secured with a mass ligature, without separating bronchus, artery, or vein.

Evarts Graham,[18] in 1923, introduced the technique of cautery pneumonectomy for the treatment of bronchiectasis in an attempt to provide a safer surgical treatment for the disease. This procedure was performed in multiple stages. At the first operation, a skin and muscle flap was raised and several ribs removed. Inspection of the pleural cavity was performed to determine if the lung was densely adherent. If it was not, adhesions were induced by gauze packing or any of the standard methods. After a suitable interval, usually about ten days, the first cauterization was done. With a large soldering iron heated to a red heat, an excavation was made into the lung tissue. This was repeated on several later occasions, attempting to excise diseased tissue and reverse bronchial drainage to the outside through the development of multiple bronchial fistulae. The results were good, and by 1930 Graham reported 66 per cent definitely improved with an operative mortality of only 11 per cent.[20] However, convalescence was prolonged and persistent bronchocutaneous fistulae frequently developed.

In 1927 Whittemore[58] described a modified technique of cautery excision. He first freed the involved lobe and exteriorized it on the chest wall by suturing it to the muscles of the chest wall,

sutures being placed as near the root as possible. Cautery excision of the exteriorized lobe was performed at a later date, or an elastic tourniquet was placed around the root of the lobe at the original procedure and the lobe allowed to slough away gradually. It is interesting to note that similar procedures were performed for accidental injuries in the fifteenth and sixteenth centuries.

Harold Brunn[4] of San Francisco in 1929 reported the first encouraging results in the surgery of bronchiectasis. Returning to a one-stage technique, he performed six lobectomies with four cures, one improvement, and one death. The lobe was carefully dissected down to the hilum. The pedicle was managed by clamping it between large, right-angle clamps and placing interrupted interlocking sutures about the pedicle between the clamps. These were tied after the proximal clamps were removed. The lobe was then excised distal to the ligatures with the cautery.

In 1932 Brunn[5] modified this technique by placing a temporary elastic ligature around the pedicle to control the stump until hemostasis was secured. The lobe was then excised distal to the elastic ligature. The bronchus and blood vessels were ligated separately, and the temporary ligature then slowly removed. The stump was closed by inverting it with a running lock stitch of chromic catgut.

The hazard of the temporary elastic ligature prompted Shenstone[52] of Toronto, in 1932, to introduce his tourniquet.

> When the diseased lobe has been completely freed, a snare of heavy cord is passed around it as near as possible to the mediastinum and the loop drawn tight in the instrument designed for the purpose. We believe it to be a better method of controlling the pedicle than any other we have seen described, and nothing has given us such a feeling of comfort and safety in the operation as this simple device. Without devitalization of the tissue, it controls the blood supply, obstructs the bronchi, provides a solid support during the suture of the pedicle, and in addition, can be used in limiting mediastinal movement during the operation.

Stenstone reported 16 lobectomies using this method with three deaths.[53] Six were considered cured, two improved, three unimproved, and two still hospitalized at the time of the report.

Thus, the 1920's saw the slow but progressive development of pulmonary surgery. New techniques were introduced, endotracheal anesthesia gained wide acceptance, x-ray diagnosis of the chest improved, and blood transfusions began to be used to compensate for blood loss during surgery. Slowly, mass ligation of the

hilum was replaced by individual ligation of the vessels and bronchi. The stage was being set for performance of complete pneumonectomy and subsequently the attack on lung cancer, a hitherto untackled disease.

Rudolph Nissen[39] of Berlin, is generally considered to have performed in July 1931, the first successful removal of an entire lung. The patient was a 12-year-old female with bronchiectasis of the left lung. The operation was performed in two stages. Portions of the third and fourth ribs were resected, phrenicectomy was performed, and pneumolysis of part of the lung effected. The patient's condition became poor so that operation was interrupted at this point, and the second stage performed 14 days later. The entire lung was freed to the hilum and a very tight rubber tube and several silk sutures tied about it. After 14 days the entire lung sloughed away, and after eight weeks only a small bronchial fistula persisted. The patient was alive and healthy six years later.

In November 1932, Cameron Haight[22] of Ann Arbor, Michigan, performed the second successful pneumonectomy in a 13-year-old girl with bronchiectasis. He used a technique similar to the one employed by Nissen and also achieved a good result.

A milestone in thoracic surgery occurred on 5 April 1933 when Evarts A. Graham[19] of St. Louis performed the first successful one-stage pneumonectomy for lung cancer. The patient was a 48-year-old physician who had a squamous cell carcinoma of the left upper lobe bronchus. The proximity of the carcinoma to the lower lobe bronchus made complete pneumonectomy necessary. Hilum ligation was performed by the method of Harold Brunn, but the bronchial closure broke down several days postoperatively, necessitating a thoracoplasty. A partial thoracoplasty had been done during the initial procedure. Recovery was satisfactory, and the patient is alive and well today, actively engaged in the practice of medicine.

Rienhoff[47] and Overholt[40] also performed successful one-stage pneumonectomies in 1933, completing the series of lung resections which proved the feasibility of pneumonectomy in man. Rienhoff, of Baltimore, in July 1933, successfully removed the left lung of a 3½-year-old girl for fibrosaroma. For the first time pneumonectomy was performed by individually ligating and dividing the broncho-vascular structures. Rienhoff did not perform a thoracoplasty as Graham had done. The matter of obliterating the thoracic space created by total lung removal was the subject of much

discussion at this time, and Rienhoff pointed out that the thoracic space fills up with serum which coagulates, forming a space-occupying substance, and thoracoplasty is therefore not necessary. Overholt in 1933 performed the first successful resection of a right lung, in a 35-year-old woman, for carcinoma.

Following these successful cases, pneumonectomy became a more frequent procedure, operative mortality gradually decreasing as experience and refined techniques increased. By 1939 Overholt[41] was able to collect 104 pneumonectomies from the literature since Nissen's in 1931. Of these, 46 were for benign conditions and 58 for malignant ones. The over-all operative mortality was 47 per cent with the mortality for malignant disease three times greater than that for benign conditions. Overholt's own series of 22 cases represented an improvement, his mortality rate being only 22 per cent. As pneumonectomy became a standard procedure, operative mortality continued to drop until today it is approximately 1-5 per cent.

In 1939 a further advance in pulmonary surgery was recorded by Churchill and Belsey.[8] It had long been known that the anatomical unit of the lung is the segment and not the lobe. Each lobe can be subdivided into segments based on the characteristic distribution of the bronchi and their patterns of branching. Churchill and Belsey were the first to approach the lung surgically along these lines. They removed the lingula of the left upper lobe for bronchiectasis, attempting thereby to conserve as much normal pulmonary tissue as possible. The segmental intralobar division became more widely recognized as the anatomical unit, and today any segment of the lung can be successfully excised.

About this time chemotherapeutic agents began to be introduced. Coupled with the subsequent advent of antibiotics, these new agents tremendously influenced the further progress of thoracic surgery. Diseases that formerly were major problems became rarities. Operations previously carrying high mortality and morbidity rates became, in a short time, safer standard procedures. Most significantly influenced were the incidence and treatment of acute and chronic empyema, lung abscess, and pulmonary tuberculosis.

Acute empyema, in spite of the progress of thoracic surgery, had continued to appear, usually as an aftermath of pneumonia. Abraham Wilensky[59] of New York City reported in 1915 on 299

cases of acute empyema over a ten-year period. He concluded that "empyema is a very common disease and occurs much more frequently in children than in adults." Average mortality for his series was 28 per cent, varying from 50 per cent in infants to 18 per cent in adults. With the advent of antibiotics, pneumonia was arrested before acute empyema collections could occur, and today acute empyema is a rarity. Occasional cases do occur, most often as postoperative complications, and are usually treated by open drainage. Chronic empyema, because of the disappearance of acute empyema, has also become mainly a matter of historical interest. The introduction of enzymatic debridement using the bacterial product, streptokinase-streptodornase, has also prevented the appearance of many cases of chronic empyema.[55]

The incidence of lung abscess as well as its therapy has considerably changed during recent years. Lung abscess, like acute empyema, has become an uncommon disease. More efficient methods in anesthesia and early antibiotic treatment of pneumonia have contributed to this marked decline in the incidence of the disease. Whereas most cases were previously treated by surgical means, usually pneumotomy, antibiotics now permit conservative treatment of most abscesses with a 75 per cent chance of cure. This is in striking contrast to a series of cases reported by Sweet from the Massachusetts General Hospital covering the period from 1938 to 1942.[54] He reported 23.3 per cent chance of cure using conservative medical measures, mainly bed rest and postural drainage. Only 5 of the 45 cases so treated also received sulfa drug chemotherapy.

The treatment of pulmonary tuberculosis also came under the influence of antibiotics. Until 1940, largely through the work of John Alexander, an American thoracic surgeon, collapse therapy and thoracoplasty comprised the surgery of pulmonary tuberculosis. Resection was hazardous and attempted infrequently, usually with poor results. In 1943, Churchill and Klopstock[9] in a revealing report reviewed the status of resection for pulmonary tuberculosis, citing six successful cases of their own. Thereafter resection gained new adherents, and with the advent of streptomycin and other anti-tuberculosis drugs there has occurred a trend toward resection as a major form of therapy for the disease.

Within fifty years surgical treatment of pulmonary diseases has advanced from mere drainage of abscess cavities to pneumonec-

tomy, lobectomy, and segmental resection. No portion of the lung is exempt today from exploration or resection. Pioneer thoracic surgeons have been able to view, within their lifetimes, this slow but steady development of a hitherto unknown and feared area of surgery. What the future holds whets the imagination. What the past has brought may best be summarized by a vision expressed by Howard Lilienthal in 1916:[23]

In the early days of operating for appendicitis, cholecystitis and many other surgical conditions, we began with the most hopeless cases, gradually winning our way until we arrived at the present gratifying stage, when early diagnosis is followed by prompt and successful surgery. So far we must be satisfied to operate in the worst and most hopeless cases of lung suppuration, where there is little to lose and much to gain. Later, with improvements in the techniques, we may find that lung surgery is no different from other surgery and that we shall operate in the early cases, with greater promise of cure.

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Bernard Connor (1666-1698)

And His Contribution to the Pathology of Ankylosing Spondylitis

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Introduction

BERNARD Connor's early contribution to the pathology of ankylosing spondylitis has been recognized for some time.[1,2,3,23,27,33,34,37,44] However, other than a short but excellent section in the *Dictionary of National Biography*,[43] and a review of some aspects of his contributions with some biographical data,[29] there has been little study of his life.

He was a youth of considerable charm and attraction who had an exciting though brief career. He was acquainted with many interesting medical personalities of the seventeenth century and was extensively travelled. The present short account contains a biography of his life with some comments on the people he met and the places he visited and a description of his contribution to pathology.

Biography

Bernard Connor or O'Connor was born in the year 1666** in the county of Kerry, Ireland. Because of the destruction of large numbers of records of this time in Ireland, it has been difficult to obtain genealogical data. There is some evidence, however, that Connor may have been a brother of Thomas O'Connor, a prominent Dublin physician and potential claimant to the Chieftainship of the O'Connor Kerry about whom information is available.[33] Bernard Connor is referred to in the manuscript, *Kerry Clans and Confiscations*, written in 1854 by the Reverend Charles James O'Connor Kerry,[28] but his exact relationships continue obscure. From his will, still preserved in the Principal Probate Registry, Somerset House,[19] it is known that he had more than one sibling (" . . . the largest diamond ring to be sold and the value given to my younger sister . . .") and that a cousin, Charles Connor, was living in London at that time.

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** In his funeral oration[24] delivered in 1698, it is stated that he died in his thirty-third year. The entry in the Medical School at Rheims[35] gives his year of birth as 1666.

He was educated privately at home but was unable to secure higher education in Ireland or England. Therefore, in about 1686 " . . . il sortit de son pays à l'âge de vingt ans et vint en France pour y étudier en médicine et chercher les moyens de s'avancer."[31]

He attended the schools at Paris, Montpellier, and Rheims and received his M.D. from Rheims in 1693. The date 1691 given in the *Dictionary of National Biography*[43] is probably incorrect. In the archives of the medical school of the University of Rheims there is a manuscript by Raussin, Dean of the Faculty of Medicine in 1756,[35] which contains a listing of the medical graduates with the date of their theses. Listed under the O'Connors is "O'Connor Bernard, Irlandis, 18 Septembre 1693, né dans le comte de Kerry en 1666. . . ." There is no O'Connor listed for 1691. The date used in the *Dictionary of National Biography* may have been derived from a paper in the British Museum, published in Paris, which is either the thesis or a publication gleaned from it.[6] This bears the date 1691 in Latin numerals but a knife cut has removed further writing from this line (Fig. 1.). It is this paper which contains the first description of the spondylitic skeleton of which more will be said later.

There were a surprisingly large number of Irishmen at Rheims at this time. Of the 3,322 medical doctorates granted between 1550-1793 no less than 654 were granted to Irishmen. Among the ten O'Connors listed there were two, a "Joaness Bernardus" (1777) and "Bernardus" (1787) both of county Kerry, who were probably related to the Bernard O'Connor with whom we are concerned.[26]

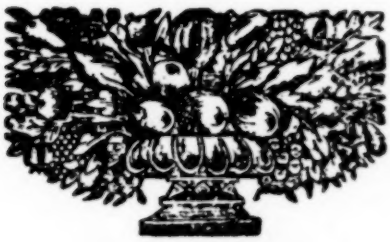
It was around this time that O'Connor decided to delete the "O" from his name. In the Paris paper[6] the "O" has been removed on the title-page by a knife cut. In a publication of the following year[7] the same alteration has been made. This is seen in the copy in the British Museum as well as the copy which the author presented to the library of the Royal Society. In a book published in 1695,[8] the name "Connor" was printed in the first instance with no alterations on the title-page. One may surmise that sometime between 1694 and 1695 he decided to alter his name and effected these changes on the title-pages of his previous publications. His reasons for doing this are obscure. Perhaps it was related to his desire to practice in England or to his political activities which will be discussed below.

d. 408.

LETTRE
ECRITE
A
MONSIEUR LE CHEVALIER
GUILLAUME
DE WALDEGRAVE,
PREMIER MEDECIN
DE SA MAJESTE' BRITANNIQUE.

CONTENANT UNE DISSERTATION PHYSIQUE ſur la continuité de pluſieurs os, à l'occaſion d'une fabrique ſurprenante d'un tronc de Squelette humain, où les vertebres, les côtes, l'os Sacrum, & les os des Iles, qui naturellement ſont diſtincts & ſeparez, ne font qu'un ſeul os continu & inſeparable.

Par Mr. BERNARD CONNOR, *Docteur en Medecine, & Anatomiſte.*



A PARIS,
Chez JEAN CUSSON, ruë ſaint Jaques, à l'Image de ſaint Jean Baptiſte. *Avec Privilege du Roi.*
M. DC. XCI

FIG. 1. The title-page of the article containing the first description of the skeleton with ankylosing spondylitis. The "O" has been removed by a knife cut from in front of Connor. An alteration has also been made on the date line. (Photograph courtesy of the British Museum.)

During his seven years' sojourn in France (1686-93) he received recognition from the medical profession and even from the French Court itself. In his thesis[6] he notes that he had students in anatomy, and several of his publications bear the following description under his name:[7,8]

> ". . . doctore Medico & Anatomico è Regia Medicorum Camerae Regiae Parisiensis Societate."

It was through his court connections that he made the acquaintance of the two sons of the High Chancellor of Poland who were visiting the country. There were strong connections between the Polish and French kingdoms at this period. The wife of the Polish monarch was French and he received a subsidy from Louis XIV.

In company with the two Polish nobles, Connor travelled to Italy, going as far south as Naples, perhaps visiting Sicily as well.[47] He attended the lectures of the anatomists and physiologists Malpighi, Bellini, Redi, and others. In later years he lectured on their discoveries. In Naples he investigated a crypt where the dead were said to rise to life and in 1694 published his observations.[7] Within the covers of this same book is a description of an eruption of Vesuvius. Since no great eruption is recorded during the years 1693-94, much of his information must have been hearsay.

He continued on to Venice which was at that time a rich commercial city, the main entrepôt between the East and southern Europe. Here, Connor encountered William Legge, Lord Dartmouth, who had fallen ill and was not responding to the ministrations of the local physicians. Connor tells us that he successfully treated him[16] and conducted him to Padua to convalesce under the care of his companion, the Earl of Kildare.

From Italy he proceeded with his Polish friends through the Tyrol, Bavaria, Austria, and down the Danube by ship to Vienna. He rested at the court of the Emperor for some time and then continued through Moravia and Silesia to Cracow, the capitol of Poland. He was joined there by a large company of voyagers and they travelled to Warsaw where the King was holding court.[16]

Poland was at that time a wild, remote country only rarely visited by Western Europeans. It was a monarchy of a unique sort; the nobles elected a king who reigned until his death but did not pass on the sceptre to his heir. The large majority of the in-

habitants were peasants and included groups of Cossacks and other nomadic or semi-nomadic people. The incumbent at the time of Connor's visit, John Sobieski, had gained prominence as a consequence of a brilliant military career. In 1667 at the head of an army which he had created at his own expense, Sobieski defeated the combined Turkish, Tartar, and Cossack forces. He again defeated their ancient enemy, the Turk, in 1673 and in the following year, when the Polish throne became vacant, he was elected by the nobles to fill it. His greatest achievement was the rescue of Vienna from the Turks in 1683 which effectively halted the further spread of Islam into Christendom.[22,36]

When Connor arrived in Poland sometime in 1694,* the king was already in poor health. Connor had received an introduction to the Venetian minister in Warsaw, Hiernonomo Alberto de Conti, through the courtesy of Conti's wife, Lady Margaret Paston, who was the sister of William, Earl of Yarmouth. While at the court, Connor's medical reputation was enhanced by an astute diagnosis. The sister of Sobieski fell ill, and the King's private physician, the Italian, Jonas, diagnosed "the ague." Connor opined that she had an abscess of the liver. When she died a short time later his view was confirmed at autopsy. He was soon appointed physician to the King and attended him in the illness which in 1696 finally led to his death.

In *The History of Poland*[16] Connor states, "When I came first into this Nation, my chief Design was to converse with Physicians and other Naturalists, to improve my knowledge in the practice of Physick and in Natural History. . . ." However, he found that there was little to learn in medicine and instead he busied himself studying the history and customs of the country.

In addition to formal historical material, he included in the *History* many bizarre observations. He gives an account of the fire and serpent worship that existed in Lithuania before its conversion to Christianity in the fourteenth century and which still persisted in the remote sections of the country. He relates his observations (and includes an illustration) of children who had been found in the woods by bears and had been raised and suckled by them. He also had much to say about the Cossacks and the Tartars who inhabited part of the country. They were horsemen

* In his book, *The history of Poland*[16] Connor states that he had not ". . . been in it [Poland] a full twelve month." He left the country on 11 November 1694.

and warriors and in every way "unacquainted with luxury, only they love aqua-vita extremely."

Late in 1694 he was entrusted with the duty of escorting the daughter of the King, Princess Teresa Cunigunda, to Brussels where she was to marry the Elector of Bavaria. They departed on 11 November with a large company and proceeded through Posnan, Frankfort, and Berlin to Wesel-on-the-Rhine where they were met by the royal suitor. He was so taken with his intended bride that he insisted that the marriage ceremony be celebrated forthwith. This was done. A short time later the party continued on to Brussels, arriving on 12 January 1695. During this long journey, Connor continued to question his Polish companions about their country and much of the information thus gathered was included in his book.

After remaining in Brussels for a short time, he left the Princess in the care of the physician of the Elector, Dr. Pistorini, and proceeded via Holland to London in February 1695.

It may be presumed that he had by this time been received into the Established Church of England for, as a consequence of the Test Acts of 1673, he would not otherwise have been permitted to attend the universities or to practise in London. He took up residence in Bow Street, Covent Gardens, which at that time was a fashionable district of the city. Strype, writing in 1720,[46] states that, "The street is open and large, with very good houses, well inhabited, and restored unto by gentry lodgings: as are most of the other streets in this parish."

Among Connor's neighbors was John Radcliffe who lived on this street from 1687 to 1714. Radcliffe (1650-1714) was one of the most celebrated and successful practitioners of all time, and much has been written about him. He was frequently called to see the British royal family and on one occasion received 1200 guineas from King William after curing a sick nobleman. When Radcliffe was being consoled by his friends on the loss of 5000 guineas in a business venture, he said, "I would only have to go up 250 pairs of stairs to make myself whole again."[43] He had been appointed physician to Princess Anne of Denmark in 1686 but later offended her by neglecting to visit her when sent for, saying that her distemper was nothing but the vapours. He had always supported scientific investigation during his lifetime, and on his death much of his considerable estate went towards the establishment of a hospital and library at Oxford which still bear his name.

Connor dedicated a letter in his book, *The History of Poland*[16] to Radcliffe and in a letter which he wrote to Hans Sloane on 28 May 1695[11] he requested that Sloane inform Radcliffe about the dedication.

Some information on this phase of Connor's life is gained from two letters which he wrote to Sloane who was then Secretary of the Royal Society.[11,17] The circumstances which led to their preservation is interesting.

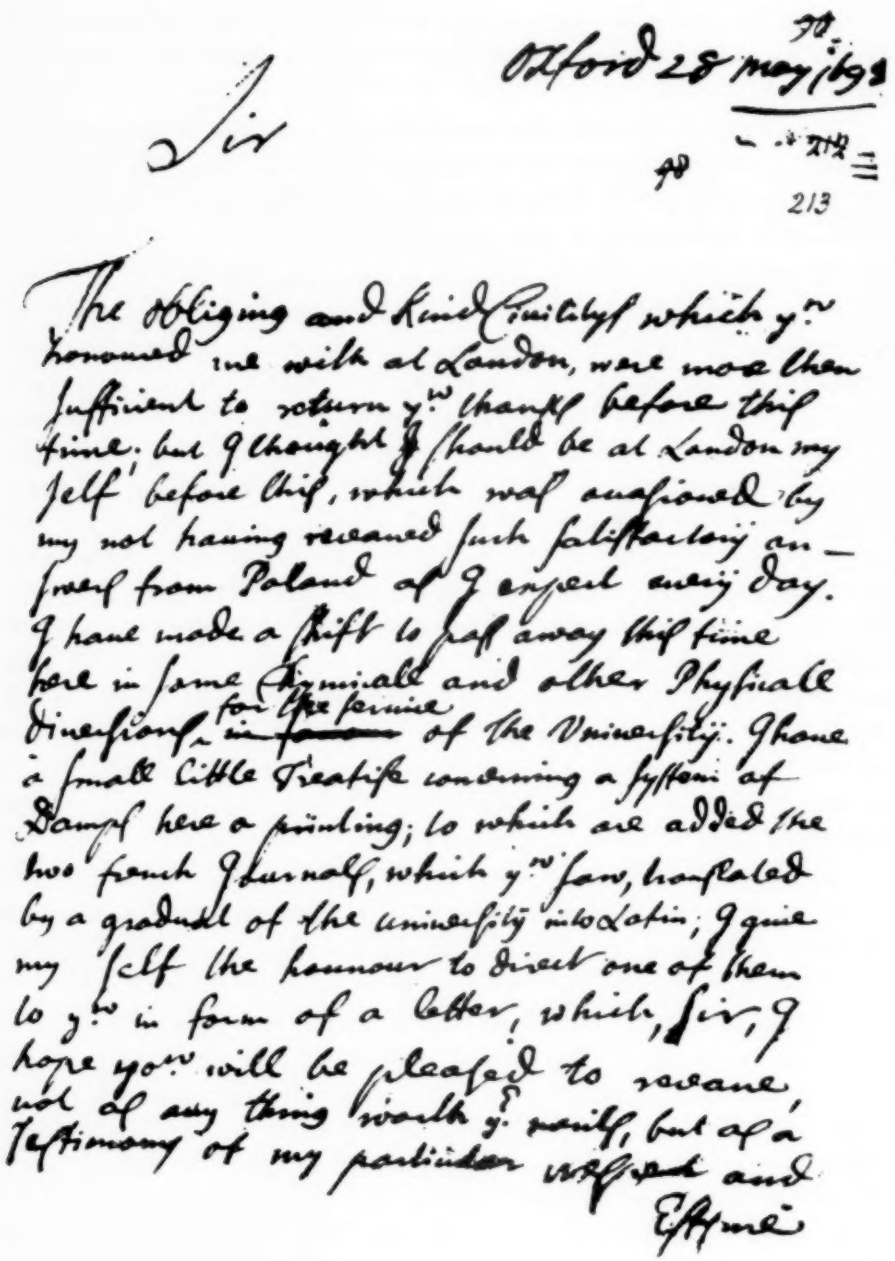
Sloane had a brilliant career in science and medicine. In 1687 he had accompanied the Duke of Albermarle to the island of Jamaica in the West Indies when that nobleman became the governor there. In addition to his duties as physician to the governor, he made a collection of 800 specimens of plants. After his return to England he became a Fellow, Secretary (1693-1712), and later (1727-41) President of the Royal Society. While occupying these offices he had an extensive correspondence with the physicians and scientists of this period. His collections and correspondence were carefully preserved and on his death in 1753 were placed in the hands of public trustees. The collection (along with several others) became the basis of the British Museum of Natural History, and the correspondence, still preserved in the library of the Museum, has become source material of the utmost importance in the medical history of the period from the time of Charles II to that of George II.[43] The first of the letters which Connor wrote is shown in Figure 2.

In one of these Connor informs Sloane about his French article on the spondylitic skeleton. We may presume that it was this notification which led Sloane to effect the translation and publication of the article in the *Philosophical Transactions* of 1695.[9]

Connor did not remain in London long but proceeded to Oxford where he spent the winter and spring of 1695. There are no records in the archives of the University to authenticate his visit but this is a poorly documented period in the University's history.* His name does not appear on any of the college rolls, from which we can be sure that he did not receive a degree. He must have been in Oxford on 28 May 1695 for one of the letters to Sloane was dated from Oxford on that day.

He remained in Oxford long enough to publish a small treatise in Latin.[8] This was a series of four separate essays bound in the

* The available records were searched through the courtesy of I. G. Philip, M.A., Deputy Keeper of the Oxford University Archives.



Oxford 28 may 1695

Sir

213

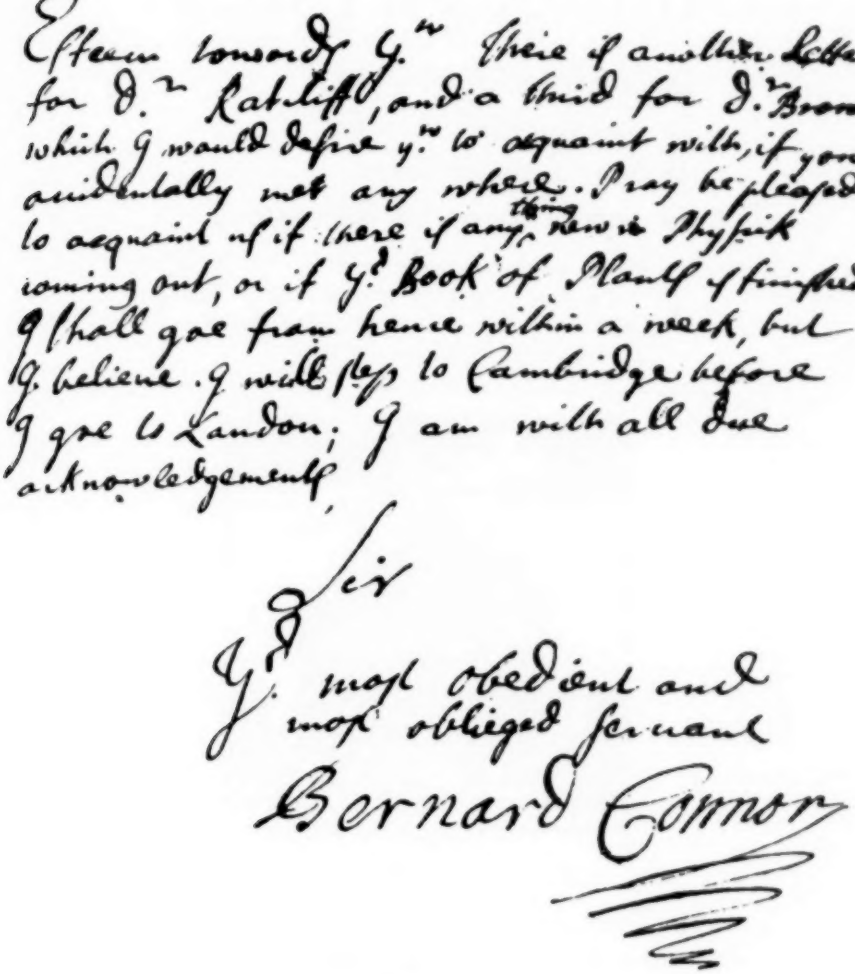
The obliging and kind Civilityes which y^r^ honoured me with at London, were more then sufficient to return y^r^ thanks before this time. but I thought I should be at London my self before this, which was occasioned by my not having receaved such satisfactory answer from Poland as I expect every day. I have made a shift to pass away this time here in some Chymicall and other Physicall diversions for the service of the University. I have a small Little Treatise concerning a System of Damps here a printing; to which are added the two french Journalls, which y^r^ saw, translated by a graduall of the university into Latin; I give my self the honour to direct one of them to y^r^ in form of a letter, which, Sir, I hope you will be pleased to receave, not of any thing worth y^r^ perusall, but of a Testimony of my particular respect and

Esteem

FIG. 2A.

FIG. 2. Letter from Bernard Connor to Hans Sloane dated Oxford, 28 May, 1695. (Photograph courtesy of the British Museum.)

same cover with a separate title-page for each essay. The first title-page, carrying the etching of the Sheldonian Theatre where the Oxford University Press was then located, is shown in Figure 3.



Esteem towards Y.ᵉ there if another letter for D.ʳ Ratcliff, and a third for D.ʳ Brown which I would desire y.ᵉ to acquaint with, if you accidentally met any where. Pray be pleased to acquaint me if there if any new thing in Physick coming out, or if y.ᵉ Book of Plants finished. I shall goe from hence within a week, but I believe I will step to Cambridge before I goe to London; I am with all due acknowledgement

Sir

Y.ᵉ most obedient and most obliged servant

Bernard Connor

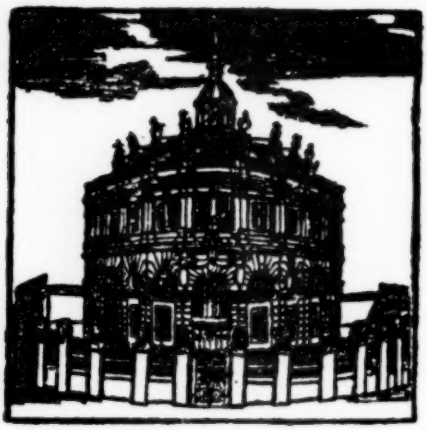
FIG. 2B.

His lectures at Oxford were well received. An unknown "Gentleman in Cambridge" wrote to him on 15 October 1695[10] and requested that he come to lecture to the students there, which Connor finally did in the summer of the following year.[16] Again, however, there is no record of his stay in Cambridge in the archives of the University.[45]

DISSERTATIONES MEDICO-PHYSICÆ.

De Antris Lethiferis.
De Montis Veſuvii Incendio.
De Stupendo Oſſium Coalitu.
De Immani Hypogaſtrii Sarcomate.

A D. *BERNARDO CONNOR* M. D.
Sereniſſimi Poloniæ Regis Medico & Regia
Cameræ *Pariſienſis* Societate.



OXONII,
E THEATRO SHELDONIANO,
Sumptibus *Henrici Clements* Bibliopolæ *An. Dom.* 1695

FIG. 3. Title-page of *Dissertationes Medico-Physicae.* The pathological description of the ankylosing spondylitis is given in the third essay. (Photograph courtesy of the New York Academy of Medicine.)

It appears that Connor did not concern himself with scientific questions alone, if we are to believe the testimony of a contemporary resident of Oxford. The last two decades of the seventeenth century were an unsettled and unhappy time in the British Isles. The relatively bloodless revolution of 1688-89 had led to the exile of James I and with him the group that favored the French and Catholics. William III, the Stadtholder of the Dutch state who had been called to head the new kingdom, was supported by the Anglicans, Dissenters, and the anti-French group. In 1692 William's position had been strengthened by his victory over the Irish and French forces under the returned James at the Battle of the Boyne. Throughout the country an immoderate suspicion of Catholics, Irishmen, and French supporters led to many unjust and hasty acts. These sentiments were particularly strong in Oxford, which in 1688 had seen the Fellows of Magdalen College turned out and replaced by the followers of James.[4] By 1695 some degree of moderation was returning to the country but it was not sufficient to remove from public suspicion a person such as Connor who was at the same time an Irishman and a Catholic (although he had presumably been converted to the Established Church on his return to England), and had been educated in France.

During this period Sir William Trumbull (1639-1716) was the secretary of state, a privy councillor, and a member of Parliament for Oxford University. His correspondence had been preserved at his ancestral home, Easthampstead Park, Berkshire, and in 1924 was published by the Historical Manuscripts Commission as the *Downshire Reports*. Among the letters received by Trumbull were two concerning Connor.[25]

Dr. Robert Gorge of Oxford, writing to Trumbull in July 1695, states that he had heard of Connor, and although Connor professed to be a Polander (which we have no evidence he ever attempted to do) he was in fact an Irishman and that he had come to Oxford not to instruct in Physick but to excite the young men to rebellion against the throne and to stir up Jacobite sympathizers.

We know that Connor changed his name in 1694 but it is hard to believe that he was attempting to hide his Irish birth by the single device of removing the "o." In a second letter Gorge states that he had received a letter in code which purported to prove that Connor was a French spy. He said that he received this in-

formation from Dr. Wallis, at that time a Fellow of the Royal Society. If this were true, it is surprizing that the fact did not prevent Connor's enrollment as a Fellow of the Society the following November.

It is difficult to evaluate these accusations. One is left with the impression, however, that Connor was more anxious to advance his medical and scientific career than to be embroiled in partisan politics.

Gorge stated that Connor had "gone off privately but whither is unknown."[25] From the date line of Connor's letters we know that he returned to his Bow Street residence to carry on his medical practice and experiments. He continued to lecture in London on his "animal oeconomy" and also on the anatomical and physiological knowledge he had acquired in France and Italy.

He had by this time become sufficiently prominent to be considered for a Fellowship in the Royal Society of London. His election is duly noted in the *Journal Book* of the Royal Society for 27 November 1695,[34] and Connor signed the first *Journal Book* of the Society under that date.[40] His only other mention in the records of the society is on 23 February 1697 when it is noted that he had presented a copy of *The History of Poland* to the Society and that it was given to Mr. Waller, the secretary.[41] A long and detailed review, presumably in the hand of Mr. Waller, is preserved in the classified papers of the Society.[39] Other than the letter in the *Philosophical Transactions*, to which reference has already been made, he apparently made no contribution to the proceedings of the Society. Connor presented several other books to the library of the Society[7,8,15] and these contain the notation, possibly in the hand of Connor, that they are the gift of the author.

Connor was further honored in 1696 by election as a licentiate of the Royal College of Physicians of London. This is noted in an entry from the Register of the College for 16 April 1696.[38] It is noteworthy that he had no compunction in taking the oath of loyalty to the Crown as was required of all licentiates. The only other entry under Connor's name is one recording the imprimatur of the College on his *Evangelium medici*.[15]

Ever since his arrival in England, Connor had written and spoken about his plan for the "Animal oeconomy," the eventual object of which was "To explain the miracles relating to human

bodies by the principles of Physick." These he had outlined in a series of published open letters[10,12-14] in *The History of Poland* and elsewhere in his publications. In this day of specialization into one narrow segment of human knowledge, one cannot but be impressed by the great size of the task that Connor had set himself.

> It is plain to me [he wrote] that a man void of all prejudice, and who considers that all the operations of bodies are performed by natural causes without miracles may be easily convinced that the causes of the disease and the true use of applications to cure them, can be rendered very intelligible so that the vulgar axiom that there is no certainty in Physick will be found most eroneous.[10]

Connor felt that some explanation should be made for the miracles of revealed religion in order that sceptics and deists might be confronted with these proofs of divine actions. He protested that he was not being irreligious in these activities but indeed that they would strengthen the faith of the unbelievers. He proposed to do this by first understanding the structure of the human body, "which I have taken apart [in] my anatomy and resolved into essential elements or minute particles by chemistry." Following this, he would investigate the "Laws of Motion," the suspension of which led to miracles. Having established this information as a consequence of experiments he planned to do, "since experiments are the only foundation we can surely build upon in Physick,"[13] he could then investigate the various miracles of revealed religion. These included the creation of Adam, the Virgin birth, the resurrection, and others. Needless to say, this led to a good deal of censure in ecclesiastical circles despite Connor's professed interest in religion. He was publicly criticised by the Archbishop of Canterbury and Connor published a letter addressed to the Archbishop defending himself against this charge of heterodoxy.[12]

Notwithstanding this censure and his rather tenuous political situation, in 1697 he published in London a Latin treatise, *Evangelium medici*[15] which embodied his radical ideas. It was an immediate sensation and had another London printing later that year, a third in Amsterdam in 1699,[18] and a fourth in Jena in 1724.[20]

Connor was now at the height of his brilliant career. He had arrived in England a stranger with serious political and religious handicaps and in a short space of three years had published sev-

eral books,* had lectured at Oxford, Cambridge, and London, had created an intellectual stir sufficiently great to receive a public censure from the Archbishop of Canterbury.

At this time, when he was "In the vigor of his youth . . . when the world was in expectation of great things from him, and when possibly improvement in Natural knowledge, reputation in his profession and advancement in his fortune filled and employed his thoughts,"[24] he suddenly died.

A good deal of information about his last days is available. In his will,[19] written shortly before his death, he designated that his cousin and executor, Charles Connor, should have a sermon preached for him by Dr. William Hayley of St. Giles-in-the-Fields, Connor's parish church, and that the address be printed.

In his sermon (later used as biographical material in *Wilford's Memorials*[47]), Hayley tells that Connor had a febrile illness, sometimes accompanied by delirium, and that he was ill for several days before dying. (No further information is given which would help in identifying the illness.) After questioning Connor about the intention of his controversial writings and being assured that he was a penitent member of the Church, Hayley had pronounced the last rites. Connor was later visited by an Irish Catholic priest who did the same for him. Hayley concluded by drawing a lesson on the necessity for being prepared for death at any time. The sermon was delivered on 30 October 1698 and Connor was interred at St. Giles-in-the-Fields three days later. The name of Dr. Bernard Connor is entered in the Registry of Deaths of the church for November 3d.**

It is not unlikely that Connor would have gone on to still greater accomplishments in his profession had he been spared a few more years. He was a man of great energy and ambition. Although some of his efforts appeared to have been directed towards gaining recognition from his colleagues and the public, he was genuinely interested in studying disease, using the new techniques of scientific experiment and rational thought that were becoming current in the seventeenth century.

* According to McGee,[30] Connor published no less than 17 medical volumes in addition to *The History of Poland* during his stay in England. However, not all of these have been traced.

** The Death Register was examined through the kindness of Dr. G. C. Taylor, the present Rector of St. Giles-in-the-Fields.

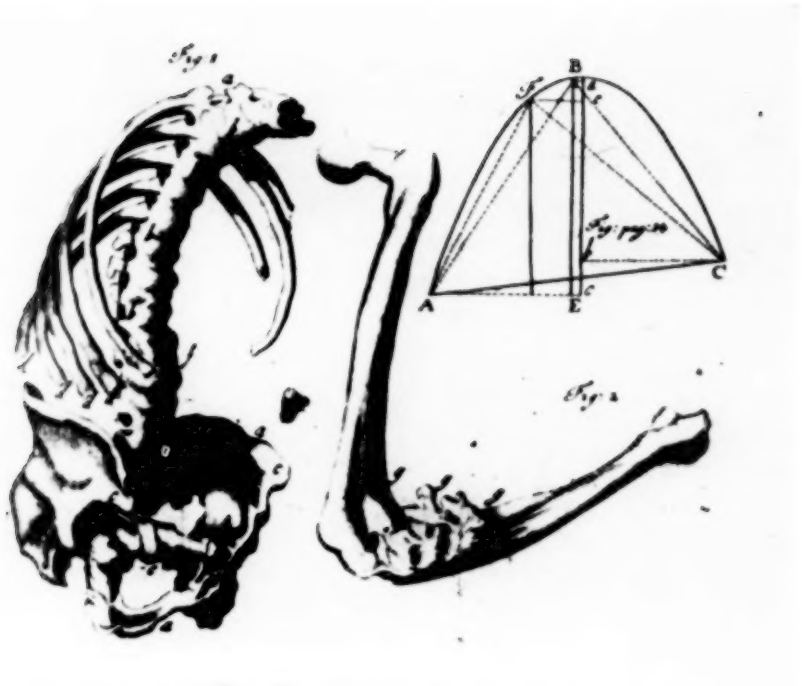


FIG. 4. Illustration from the article in the *Philosophical Transactions* published in 1695. Picture 1. is the pathological spine. Picture 2. is an ankylosed knee (from another skeleton) which might have been caused by rheumatoid arthritis or infectious arthritis. (Photograph courtesy of Dr. Charles Ragan, Presbyterian Hospital, New York City.)

Connor's Description of the Pathology of Ankylosing Spondylitis

The history of ankylosing spondylitis has recently been reviewed.[33] Although there has been some description of skeletons with abnormalities which might have been produced by ankylosing spondylitis prior to the seventeenth century, it is probable that Connor's is the first adequate account of what was undoubtedly an example of this disease. The description is contained in no less than three separate papers, in as many languages,[6,8,9] the first being the French publication of 1693.[6] The accuracy of his description makes it of contemporary interest, while his suggestions on etiology reveal some aspects of the medical thinking of that period.

Sometime prior to 1693, while Connor was demonstrating in anatomy in France, he was told of an unusual skeleton that had been found in a local cemetery. It consisted of the os ilium, os sacrum, five lumbar vertebrae, ten thoracic vertebrae, and five right and three left ribs, all firmly united at "the joinings" so that they made one uniform continuous bone. The joints between ribs and vertebrae were completely obliterated and the normal juncture was impossible to detect. The "Cartilagineous edge," that is, the annulus fibrosis of the intervertebral disc was completely ossified, although there was no evidence of joining in the center of the vertebrae where the nucleus pulposis would have been in life. Connor tested this by cutting the vertebrae through at the commissures and "found this uniting did not enter above two lines deep."[9] This conforms with the modern view that it is the annulus fibrosis calcification and not the longitudinal ligaments which causes the "bamboo" appearance of the spine on x-ray.[5] In many cases, however, the longitudinal ligaments are involved as well.

The marked curvature of the spine, characteristic of the disease, is well seen in the diagram which accompanied the article in the *Philosophical Transactions*.[9] (Fig. 4). Connor further commented on the unnatural arrangement of the direction of the ribs and their possible effects on respiration. Although there is no evidence that Connor ever saw a case when alive, he hazarded some opinions on the clinical state of the patient. He remarked that the body of the person when alive must have been immovable, that he could neither stretch, rise up, or lie down, or turn upon his side, having only the head, feet, and hands movable. Since the

ribs were fixed, it would be impossible for the patient to expand and contract his chest in the usual manner. He had previously seen that the motion of the chest was not necessary for respiration as in "hysteric fits, etc.", and observed that the diaphragm could still be completely movable. This auxiliary means of respiration could supply the patient with sufficient air if he were to utilize short, rapid breathing. Recent studies have shown that many of these patients do not have disabling cardiorespiratory symptoms and that although their air volume is considerably reduced, they can use the air available to them as well as normal subjects.[21]

The latter part of the article is concerned with some speculations on the etiology of this condition. Although his remarks seem naïve and quaint to modern ears, it should be recalled that even now we have very little more information on the etiology of this perplexing illness.

Connor dismissed the possibility that the unification occurred post-burial as a consequence of unusual soil conditions or was acquired during adult life (although his reason for rejecting the latter is not made clear). He suggested that the deformity originated *in utero* as a consequence of pressure from an abscess tumor in the womb or elsewhere which led to an abnormal development of the spine with the other changes secondary to this.

In concluding, the author raises the question of why some people should become bony in some regions of the body and others not and says that some explanation of this fact might be necessary before any understanding of the pathogenesis is achieved. This problem still remains to be solved.

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Cure by Exorcism in the Island of Cephalonia

PETER HARTOCOLLIS*

WHILE studying the psychological problems created by the great earthquake that destroyed Cephalonia in the summer of 1953,[1] the author of the present report came across an interesting phenomenon, that of a Saint believed to protect and cure the insane.

Largest among the seven islands of the Ionian Sea, on the western coast of Greece, Cephalonia has been known for its natural beauty and adventurous, seafaring people since the times of Homer. More recently, a disastrous earthquake made the island's name familiar around the world. But what only few people outside Greece probably know[2] is that the island has been for a long time the center of a strange pilgrimage. A large number of mental patients converge there every year in the hope of being cured by the island's patron Saint, Gerasimos.

In a small valley covered by olive trees, and cut off from the sea by high rocks, there lies the church of Saint Gerasimos and a convent for women, which the Saint himself established in the last years of his life in the sixteenth century. Near the convent and its church one ordinarily finds only nuns and mental patients, who are camped there all year round. But twice every year, on 16 August and 20 October, the dates when the memory of the Saint is honored, this population swells with the arrival of more mental patients and their escorting relatives as well as pious pilgrims from all parts of the island and the Greek mainland across the sea.

The day before the celebration of the Saint's memory, a silver chest containing the body of the Saint is brought and placed upright in the main church of the convent. Through a glass window, the dark and bony face and hands can be seen, the rest of the body being covered by various silver and golden offerings. All night long, people pass in front of the sacred chest and kiss the glass window that is just above the hands of the Saint. Close by, the priests read prayers in low voices, while a self-appointed chorus of

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1 P. Hartocollis, *La psychopathologie du tremblement de terre* (Une étude du comportment et des effets psychologiques produits à la suite du séisme de l'île de Céphalonie en août 1953), Lausanne, Imprimeries Réunies, S.A., 1955.

2 Dr. Henri Ellenberger of the Menninger Foundation, who inspired the preparation of this report, has discussed briefly the Cephalonia case in his article "The ancestry of dynamic psychotherapy," *Bull. Menninger Clinic*, 1956, 20, no. 6).

men chants as loudly as it can in order to be heard over the voices of the patients who have been placed by their relatives strategically throughout the church.

Next morning, after a special mass, the crowd gathers in the large yard of the convent and prepares for the litany in honor of the Saint. Just prior to the appearance of the clergy and the chest with the Saint, the patients are led in the path that the procession is to follow. They are here cast down, most of them being held forcibly, men, women, and children, fighting desperately to free themselves, crying, cursing the Saint, making obscene gestures in the direction of the church. Some of the patients are so violent that they are chained. One after another—there may be as many as fifty or eighty—all patients are made to lie down in line.

Then, at twelve noon, the bells of the church start ringing; under the burning sun of late summer the vanguard of the procession with many crosses and other religious standards appears, followed by a chorus of men chanting various songs. Finally, the sacred chest, glittering under the sun, makes its appearance. It is held by four priests all dressed in gold-ornamented robes; a few steps behind follows the bishop, holding his golden mitre and surrounded by additional priests ready to line up behind him. The official procession is followed by the sixty nuns of the convent, dressed in their black robes, each with a small cross on her forehead. Now come the pilgrims holding candles, and the procession marches out of the convent courtyard. There the procession is joined by several bands of musicians from the capital town of Argostoli.

With the bands playing a slow march and the bells of the monastery ringing, the procession marches slowly on, over a dust-covered road and through a chain formed by the mental patients lying or held down by their relatives. The idea is that the passage of the bishop and the priests carrying the sacred chest with the body of the Saint over these patients, will force the evil spirit, overcome by the exorcism and the Saint's power, to leave the patient's body. Once the chest with the Saint has passed over a patient, his relatives carry him quickly to the front of the procession, in attempt to have him passed over as many times as possible. The procession comes to an end at a place marked by a big maple tree and a well, the existence of both of which is attributed to the hand of the Saint. The patients have all been carried to this place, exhausted from the effort to free themselves, and from the sometimes brutal treatment of their attending relatives. They

are also weak from the lengthy fasting they have undergone before being brought to the procession.

Now the bands stop playing and everybody waits for a miracle to occur. The bishop prays for the mentally ill; he chants a special exorcism written by Chrysostomos, one of the sacred fathers of the Greek Orthodox church.

When prayer and exorcism are over, the bands resume their playing; the standards of the church, the crosses and the flags are raised as is the chest with the Saint's body, and the procession starts its way back to the convent, again over the long line of patients that has been formed as before.

How and when the belief in Saint Gerasimos' power over the mentally ill originated is not certain. Moreover, there is nothing definite in the Saint's life indicating that he was especially interested in the subject of mental illness. Born near the ancient city of Corinth on the Greek mainland, Saint Gerasimos belonged to a family that was descended from the Byzantine nobility. He is said to have had a broad education, based on the Greek classics, but within the framework of the Greek Orthodox Church.

After extensive travels, he reached the island of Cephalonia where he established himself as a hermit. Rather soon, but not without some friction and misgivings on the part of the island's inhabitants, he made such a profound impression on them that many an influential family entrusted their daughters to him to teach them Christian piety and wisdom. Whereupon he founded a women's convent and proceeded in developing the surrounding wasteland into a most successful farm. Whether in connection with his farming effort, or in pursuing some other well-meant purpose, he constructed a great number of wells—and no account of his life fails to emphasize this deed. One of these wells serves as the landmark where the exorcism for mental illness is pronounced on the anniversary of the Saint's death.

There seems to be no official account regarding the establishment of the rite that culminates in the pronouncement of the psychotherapeutic exorcism. But the fact that one of Saint Gerasimos' earliest miracles is reported to have occurred in connection with one of his wells may speak for the origin of the rite.

According to the legend, shortly after Saint Gerasimos' death a woman possessed by the evil spirit fell into one of the Saint's wells. The timely appearance of the Saint in the dreams of one of his nuns resulted in the salvation of the unfortunate woman from the

well and, according to the legend, also from the evil spirit that possessed her.

Saint Gerasimos' miraculous career must have started shortly before or just after his death in 1579. He is believed to have cured a wide range of ailments, in addition to having saved the island and its people from such things as foreign invasion, epidemics, and drought. However, the majority of the miracles reported in the literature refer, from the earliest times to the present, to cures of the mentally ill.

Today, if anyone has the right to seek the favor of Saint Gerasimos, it is the mentally ill person who would be more likely to find it. Yet even among mental patients, so the popular consensus holds, it is the so-called "possessed," known as the "Saint's patient," who would be most likely to benefit from the exorcism and the Saint's psychotherapeutic power. As for the diagnosis of such a patient, a good criterion is supplied by the success of the exorcism.

A "possessed" one is necessarily violent. He can be noisy or silent, but he will always be actively negativistic; in general, he is uninhibited and resistant to external control. All this, according to popular belief, can be attributed to a demon, an evil spirit that inhabits the body and destroys or, rather, takes possession of the soul of the individual—man, woman, or child. The demon has his own identity, is not merely another facet of the patient. Thus patients conversing among themselves refer to their old identities in the third person. This is shown in the following dialogue between two women patients reported by Loukatos.[3]

"Oh boy! where did you find that fat one you've gotten into?"

"Brother, don't ask me! you don't know how long I looked around in order to find her. I caught her one afternoon in an olive tree and I went into her, ha, ha, ha."

In a treatise about the life and miracles of Saint Gerasimos, a clergyman[4] describes two other cases that the power of the Saint reportedly cured as follows:

> Also the terrifying miracle that occurred on August 15th, 1924, pertaining to the cure of two women, Ann Th. Danatou, from the village Vari, and Myrianthi K. Phoka from Tsakarisianou, seized by really beastly demons, created a great impression and sacred emotion in all pious pilgrims. These demons, manifesting themselves with the force and the wildness of

[3] Δημ. Σ. Λουκάτου, Κεφαλονίτικη Λατρεία. Ἀθῆναι 1946.

[4] Γερ. Χ. Ποταμιάνου, Βίος καί Ἀκολουθία τοῦ Ὁσίου καί Θεοφόρου Πατρός ἡμῶν Γερασίμου. Τυπογρ. Α. Ζ. Διαλησμᾶ, Ἀθῆναι 1925.

desert beasts, were called as they themselves admitted, the one belonging to poor Ann, Trikeris, and the other, that of the unfortunate Myrianthi, Drakontios; and they swore, cried, yelled, laughed, bit and groaned, in short they tortured sadly the bodies of the two unhappy creatures. . . . [The names of both demons, in the example, are masculine; Trikeris means "three-strings," and Drakontios means "dragon-like."]

Patients become especially violent as the procession with the priests carrying the body of the Saint approaches them. They try desperately to free themselves, shouting wild cries or insulting and threatening words to the priests and to their Saint, to whom they usually give the name "Kapsalis," meaning "the burning one." The following are characteristic expressions of a patient confronted by the Saint's power: "Kapsali, you burned me." "I am going to break down your chest, bad old monk!" "Go away, you bishop looking-like-a-goat with your goat beard." "Go away you goat-looking priest!" "May the devil get into you!"

The pronouncing of Saint Gerasimos' name, itself, is often enough to create a violent reaction on the part of the patients; and, as if purposely, priests and chorus follow the practice of raising their voices in a provocative way every time that they pronounce the name of the Saint, both in church and during the litany. But, at the very moment that the sacred chest passes over a patient, calmness prevails, the patients become motionless and speechless, until they have been passed. Then they are likely to become active and violent again.

The belief is that every time a cure takes place, it is marked by some extraordinary occurrence, such as the breaking of a glass window in the church, the violent shaking of one or all of the holy lights or the extinguishing of these lights as if by a sudden wind. Such a mischievous act can be threatened by the demon—an indication that he is close to defeat—as follows: "Kapsali, I'll get out of this guy here [the patient], but I am going to break your holy light."

A more serious damage, reported to the author by an eye witness, was the breaking of a heavy branch of the Saint's tree. This occurred at a time when many people had climbed the tree in order to see the approaching procession better.

Most of the Saint's patients seem to be well oriented as to place and, although rejecting their old identity, they retain an acute interest in the happenings around them. They are keenly aware of the whereabouts of the Saint's body, of the ceremonies in and outside the church, as several reported episodes indicate. Once, when a bishop considered disreputable came to take part in the

Saint's celebration, the patients kept interrupting the religious performance with pertinent accusations; and when at the end of the mass he asked the approval of the audience for the naming of a reputedly unworthy layman to the priesthood, the patients cried in chorus: "He is unworthy like you."

The islanders also believe that the possessed have prophetic powers. When a woman laughed at a patient, he turned to her and, pointing at her little daughter, predicted that the girl would soon be brought to the Saint as a mental patient. And, according to the person who reported the story, a school teacher, the prophesy has come true.

But whether correct or not, such reports give an idea of the mystical nature of the relationship that exists between the population and their Saint—a relationship that may explain a great part of the curative ability of the Saint.

Development and Use of the Rubber Glove in Surgery and Gynecology

CURT PROSKAUER*

ON 10 January 1834 a young physician named Richard F. Cooke, who had "dropped anchor" in Hoboken, New Jersey, sent off a manuscript on medical ethics with an accompanying letter[1] to his former teacher, "Valentine Mott M.D. Professor of Surgical Anatomy &c. Park Place, New York," who taught surgery at the College of Physicians and Surgeons, Columbia University.

Cooke, whose life and work are relatively unknown, would have been surprised had anyone told him that a few lines of his letter to Mott would prove more important in the development of medicine than all 38 quarto pages of his manuscript put together. In this letter, for the first time, we find not only the statement that "a pair of India rubber gloves would be perfectly impenetrable to the most malignant virus" (Fig. 1c), but also reference to "a very nice solution of Caioutchiouc, dissolved in Guthries spiritus of Terpentine," a sample of which he sent along with his letter (Fig. 1a).

Hardly three years before, on 8 May 1831, the chemist, inventor, and physician Samuel Guthrie of Sackett's Harbor, New York, had informed the famous Connecticut chemist Benjamin Silliman, founder and editor of the *American Journal of Science and Arts,* of his experiments on the purification of oil of turpentine, a process he had discovered a year earlier.[2] "It is, as I think, an article of considerable importance. It dissolves caoutchouc, and the solution dries rapidly, and does not continue sticky like the solution made with common oil of turpentine." In another communication to Silliman, Guthrie wrote: "Few things that have engaged my attention, have cost me so much trouble as divesting spirits, or rather oil of turpentine, of the last particle of its resin. . . . My first object was to obtain a perfect and clean solvent for caoutchouc. . . . The oil of turpentine thus prepared, with *warmth*

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[1] Now in the manuscript collection of the Rare Book Room of The New York Academy of Medicine (sign: MS 560).

[2] *Amer. J. Sci.*, 1832, *21*, 93: Art. XI.

and strong solar light, is, as I believe, a *perfect solvent of caoutchouc."*[3]

The results of Guthrie's experiments appear to have impressed Richard F. Cooke so much that he repeats Guthrie's instructions for dissolving caoutchouc. In his letter to Mott he writes: "I take the liberty of leaving with this also a very nice solution of Caioutchiouc. I dissolve it in Guthrie's spt. [spiritus] of terpentine highly rectified by the acid of sulph-acid and add a few drops of ol. of wintergreen or any other essential oil, and in certain cases ol. of Tar.—" (Fig. 1a).

And now comes his splendid suggestion:

> This if I mistake not, will become a useful material in the surgeon's hand. I have used it in phlegmonous and erysipelatous inflam[mations] with great benefit, also in sprains and bruises. . . . (Fig. 1a). When applied to a part by means of a brush or the finger the Terpentine evaporates and leaves an application, firmly and nicely applied to the most irregular surfaces. . . . (Fig. 1b) I would further add that this is convenient to use in dissecting rooms and in vaginal examinations. By lubricating the hands with it you have an insoluble pair of India rubber gloves—perfectly impenetrable to the most malignant virus. The Terpentine gives no inconvenience as it immediately evaporates. It may afterwards be completely removed, by bringing the hands together smoothly, or rubbing them with some granular substance as hair powder or Indian meal (Fig. 1c).

Here we have the first known mention of rubber gloves "in the surgeon's hand" to prevent infection by "the most malignant virus . . . in dissection rooms and in vaginal examinations."

About a decennium later, Thomas Watson (1792-1882), "Fellow of the Royal College of Physicians, Late physician to the Middlesex Hospital, and formerly Fellow of St. John's College, Cambridge," suggests gloves for antisepsis:[4] "In these days of ready invention, a glove, I think, might be devised, which should be impervious to fluids, and yet so thin and pliant as not to interfere materially with the delicate sense of touch required in these manipulations [gynecological examinations and child-birth]. One such glove, if such shall ever be fabricated and adopted, might well be sacrificed to the safety of the mother, in every labor." Watson recommended these gloves—and he may have meant rubber gloves—because of his

[3] Article VI. Remarks on various Chemical Preparations; in a letter from S. Guthrie to the Editor, dated Sacket's Harbor, N. Y. Sept. 12, 1831. *Amer. J. Sci. Art.,* 1832, *21,* 291-2.

[4] Watson, Thomas. *Lectures on the principles and practice of physic; Delivered at King's College, London.* London, John W. Parker, 1845, vol. II, p. 349. First published in the *Med. Times and Gazette,* 1840-1842.

Valentine Mott M.D. — Hoboken House Jan. 10th 1884.
Professor of Surgical & Operative Surgery —

Dear Sir The Redemption of a promise & the desire of expressing some testimony of my regard for the authority of such valuable instructions in the department of surgery, as I have been the favoured recipient, has induced me to send you the accompanying Manuscript. In Dr Mott there can certainly be nothing new advanced upon the subject of medical Ethics. Although I felt all the dignity of a discoverer — advancing upon [illegible] grounds when I wrote the Thesis. — But the novitiate must be pardoned for claiming originality occasionally when his progressive steps are constantly admonishing him upon that which is new to himself & he imagines it bears the impress of novelty to every one else. Chronicles his every reckoning. — I have omitted much of the original of this transcript. lest the whole might sweaty your patience — From what remains after the 10th page — should your time permit perhaps you might gather sufficient to induce your concurrence. That this subject may be advantageously pursued & procure a share of that devotion which in the course of my life I hope to direct to the benefit of the profession —

I took the liberty of leaving with this also a gummic solution of Colocynthine. I dissolve it in Gutta Ppt. of turpentine highly rectified by the acid of Lycopodium & add a few drops of Ol. of wintergreen or any other essential oil. in certain cases of Pain. — This Ol. I mistake not will become a useful material in the Surgeons hand. I have used it in Phlegmonous & Erysipelatous inflam. with great benefit. Also in spasmodic & tonic Pains led to the application of it from the effi-

FIG. 1a. Page 1 of the letter from Richard F. Cooke to Valentine Mott. The paragraph at the bottom of the page contains the phrases quoted in the paper.

FIG. 1b. Page 2 of the letter from Cooke to Mott. See particularly lines 2 to 4.

all these remedies either directly or indirectly consist
in arresting insensible transpiration from the sur-
face the latter two accomplish this when inflam-
tion is deeply seated. The latter when appropriately
the surface they all accomplish this but in different
ways. As the latter viz Cataplasms & Cotton: how best
suited to my present purpose I will only refer you
give by stating. That they are simple non conductors
of heat by means of the air entangled in the fibres of the
cotton & the warmth & moisture of the Cataplasms
Instead of increasing perspiration they retard it. They
retard that insensible transpiration which is increas-
ed in the ratio that heat is radiated when thus re-
tarded it assumes the form of sweat upon the sur-
face which has given rise to the delusion that
poultices cause a part to perspire. I could prove
this to your entire satisfaction by a number of ex-
periments & observations I have already instituted
but my account of these might be tedious to you. So
I must beg leave merely to state that this solution
is an absolute electrum & as such a perfect non conduc-
tor & as such answers better than any Rubefacient Lin-
cocione — of in incipient inflammation & better than
any poultice for "Fluens Bowels" which your lectures
justly estimates not exceptionable in the suppurative
stage — — — I could this subject be sufficiently
interesting to warrant the pains of your perusal I
will be happy to give in form the history of several
cases in which I have applied it & its results — I
would further add that this is convenient to use
in dissecting rooms & in diagnosis examinations.
By lubricating the hands with it you have an impen-
etrable pair of India Rubber Gloves perfectly impenetra-
ble to the most malignant poisons. The turpentine gives
no inconvenience to it immediately evaporates. It

FIG. 1c. Page 3 of the letter from Cooke to Mott. See the last several lines for quotations in the text.

dreadful suspicion that the hand [of the physician] which is relied upon for succour in the painful and perilous hour of child-birth, and which is intended to secure the safety of both mother and child, but especially of the mother, may literally become the innocent cause of her destruction; innocent no longer, however, if, after warning and knowledge of the risk, suitable means are not used to avert a catastrophe so shocking. I need scarcely point to the practical lesson which these facts inculcate. Whenever puerperal fever is rife, or when a practitioner has attended any one instance of it, he should use most diligent ablution; he should even wash his hands with some disinfecting fluid, a weak solution of chlorine for instance: he should avoid going in the same dress to any other of his midwifery patients: in short, he should take all those precautions which, when the danger is understood, common sense will suggest, against his clothes or his body becoming a vehicle of contagion and death between one patient and another. And this is a duty so solemn and binding, that I have thought it right to bring it distinctly before you.[5]

These remarkable lines appeared about five years before the Viennese obstetrician Ignaz Philipp Semmelweis published his "Höchst wichtige Erfahrungen über die Aetiologie der in Gebäranstalten epidemischen Puerperalfieber" (Highly important observations on the etiology of puerperal fever epidemic in lying-in hospitals), 1847-1848,[6] setting forth his discovery that puerperal fever was in most cases transmitted by "decomposed organic matter" on the hands of physicians and students. Like Watson before him, Semmelweis recommended rigorous hand-washing in a cal-

[5] Rubber gloves are spoken of elsewhere, for instance by Warner Wells in "Surgical practice in North Carolina. A historical commentary." (*N. C. med. J.*, 1954, *15*, 281-7). The reference by Dr. Wells is to be found in the *North Carolina Medical Journal* [old ser.] March, 1878, *1*, 168-9: Our New York Letter. New York, February 26, 1878 by [M. J.] DeR.[osset] " . . . Your correspondent witnessed a late case of ovariotomy by Dr. [F. Wood] Thomas. The practical details in the procedure may be useful to those who are interested in that line. It was at the Woman's Hospital, not in the main building, however, but in a small frame cottage on the grounds, to diminish the danger of septic influences. . . . Six assistants, one for the ether, two for manipulating the abdomen and body, one having charge of the instruments, two for the carbolic sprays—besides two nurses for handing warm water to the operator to keep his hands clean. The instruments were scalpel, grooved-director, scissors, sounds, trocars, . . . *all kept in a shallow pan of carbolized water, in charge of an assistant who wore rubber gloves to preserve his hands from the caustic effects of the acid.* . . ." See also Miss Miriam Tucker. Men of medicine. The reluctant surgeon. *Postgraduate Medicine*, 1951, *9*, 74-81. "After his graduation from St. Louis Medical School in 1846, Dr. [Timothy Loisel] Papin went to Paris to study. . . . On his return to St. Louis, Dr. Papin brought the French knowledge of the use of the obstetric forceps, and early attracted much attention and some criticism. He wrote very few papers. . . . Back in the eighties Dr. Papin was already using rubber gloves when attending infected cases of parturition to, as he expressed it, 'prevent carrying the disease to other women.'" I am indebted to Dr. Martha Gnudi, Webster Library, Columbia University, for this reference. Miss Della O. Cooper, Saint Louis, informs me that she "checked with the various members of Dr. Blair's family, who do not remember hearing anything about their great grandfather's [Papin] using rubber gloves. Mrs. Richard Boyle, Dr. Blair's sister, does remember quite well that she and the other children amused themselves by making balloons out of their grandfather's rubber glove fingers." Dr. Papin, who "did not [do] much writing," published nothing about rubber gloves; the above-mentioned rubber fingers that he used were perhaps transformed by report into *gloves*.

[6] *Ges. Aerzte Wien*, 1847-48, *4*, pt. 2, 242-4; 1849, *5*, 64-5.

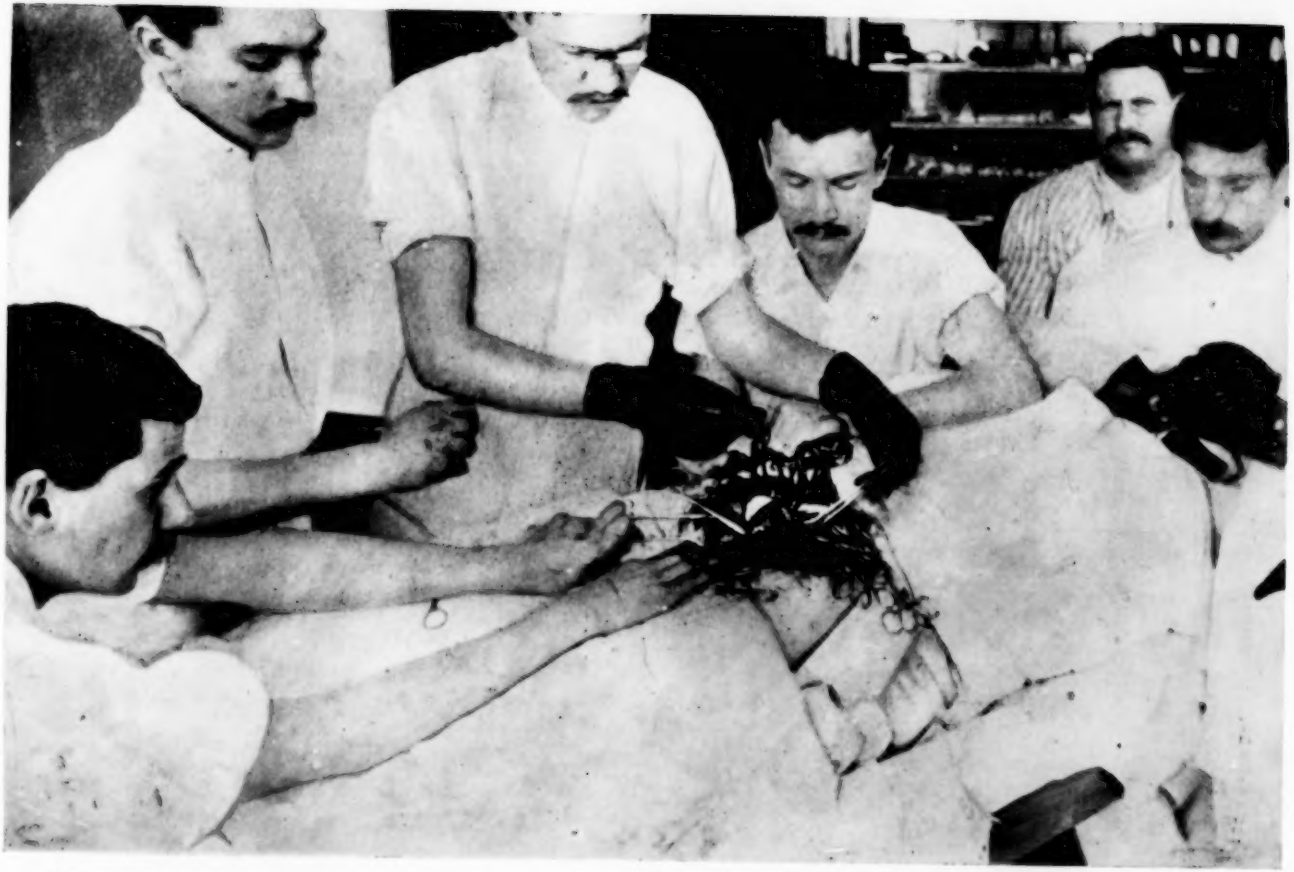

FIG. 2. A photograph of the first surgical operation in which the operator wore rubber gloves. (Reproduced from a photograph made in 1893 by Dr. James F. Mitchell)

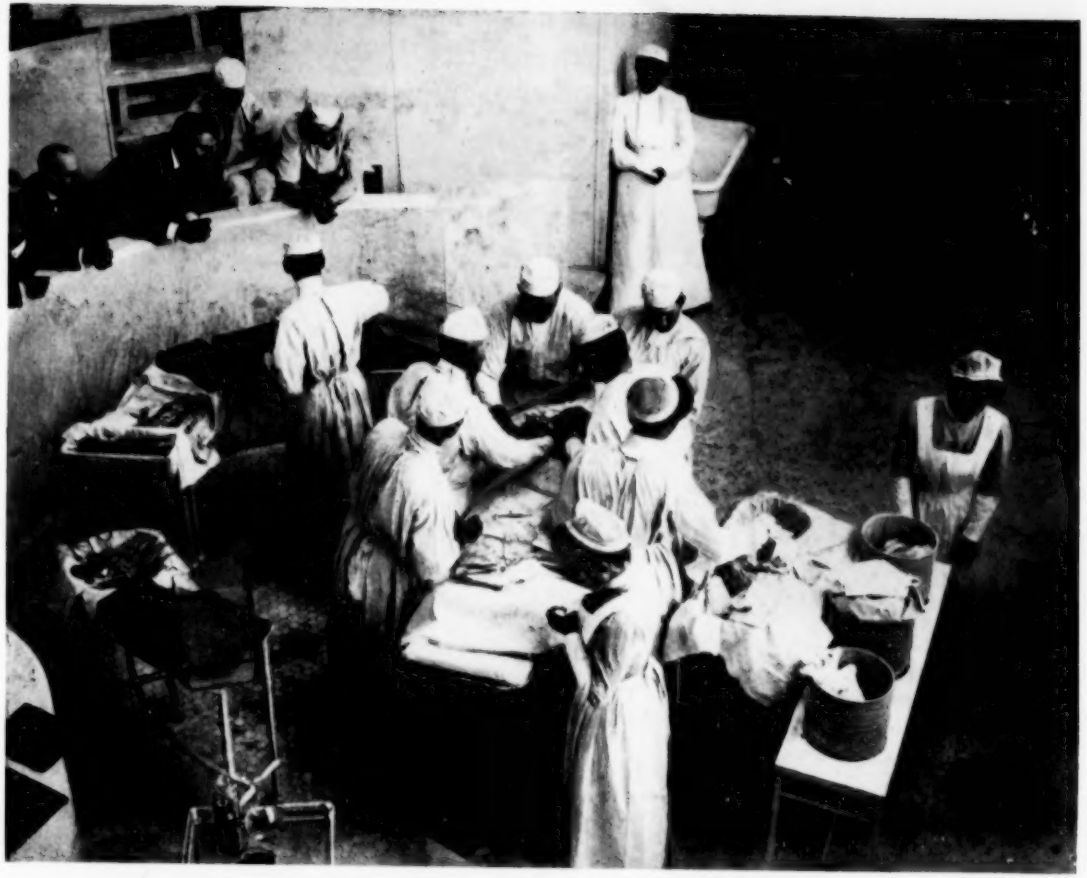

FIG. 3. A photograph of the first surgical operation in which the operator and assistants wore rubber gloves. (Courtesy of Dr. James F. Mitchell)

cium chloride solution before vaginal examination and medical care in connection with pregnancy and labor, although he did not know what transmitting agent he was destroying. It is quite possible that Semmelweis had read the famous lectures of Watson, the leading clinician of his day, which had been published not only in the journal *Medical Times and Gazette*, but also in book form; these were the most important and most popular clinical medical treatises to appear in Semmelweis's time.

Nevertheless, it took exactly half a century for Watson's dream to be realized—a practical glove "which should be impervious to fluids, and yet so thin and pliant as not to interfere materially with the delicate sense of touch required in these manipulations."

While Cook had hoped to make a glove which should protect the surgeon's hands against "the most malignant virus," and Watson wanted one to protect the patient against infection from the surgeon's hands, actual use of rubber gloves in surgical operations in fact resulted from a surgeon's compassion for the sensitive skin of his nurse's hands. Dr. William Stewart Halsted, graduate of the College of Physicians and Surgeons of Columbia University, and first Professor of Surgery at the newly founded (1889) Johns Hopkins Medical School in Baltimore, tells the story.[7] We learn that he was the man who actually introduced the use of rubber gloves in surgical operations: "In the winter of 1889 and 1890—I cannot recall the month—the nurse in charge of my operating-room complained that the solutions of mercuric chloroid produced a dermatitis of her arms and hands. As she was an unusually efficient woman, I gave the matter my consideration and one day in New York requested the Goodyear Rubber Company[8] to make as an experiment two pair of thin rubber gloves with gauntlets. On trial these proved to be so satisfactory that additional gloves were ordered. In the autumn, on my return to town, the assistant who passed the instruments and threaded the needles was also provided with rubber gloves to wear at the operations. . . . This assistant was given the gloves to protect his hands from the solution of phenol (carbolic acid) in which the instruments were submerged rather than to eliminate him as a source of infection." According to Halsted, the assistants in time became "so accus-

[7] *J. Amer. med. Ass.*, 1913, *60*, 1123-4.

[8] The Goodyear Glove Rubber Division of the United States Rubber Company has informed the writer that they cannot find any photographs or drawings of the first rubber gloves manufactured for Professor Halsted. "Evidently through the years this material was lost or disposed of. . . ."

tomed to working in gloves that they also wore them as operators[9] and would remark that they seemed to be less expert with the bare hand than with the gloved hands." Dr. Joseph Colt Bloodgood, Halsted's house surgeon[10] called by the staff "Bloodclot,"[11] who first made this comment ". . . was the first to wear them, invariably, when operating." (Fig. 2)

In Dr. Bloodgood's report on hernia operations[12] in the Johns Hopkins Hospital (1899), his chapter on "the wearing of rubber gloves by the operator and assistants" gives these figures:[13]

The following study of the suppuration of the wound after operation for inguinal hernia is chiefly of historical interest, to the operator as well as all assistants, because since . . . the use of rubber gloves, the suppuration of the wound has been almost eliminated. Between February, 1897, and January, 1899, 1 year and 11 months, there have been 181 operations for inguinal herniae with only one case of suppuration[14] [whereas when gloves were not worn, in 1891-1892], there were 26 operations for hernia with 9 suppurations (29 per cent), 5 acute infections, 3 late infections, and 1 secondary stitch abscess.

These gloves have been worn by the operator with very few exceptions, and by all the assistants without an exception (Fig. 3) from February, 1897, to the present time, June 1899. . . . The writer was the first as operator to wear gloves as a routine practice in practically all clean operations. . . . The importance of wearing gloves, especially by all the assistants, and even by the operator, can easily be appreciated. The assistants come from the ward visit, where they may have handled all sorts of infections, directly to the operating-room. It is impossible in a large surgical clinic to isolate or to have dressed by assistants who do not come to the operating-room all cases of granulating and infected wounds. Many of these assistants operate on infected cases in the out-patient department; some assist at autopsies and work in pathology and bacteriology. Their hands and fingernails must always contain all sorts of bacteria, and now and then perhaps very virulent streptococci and staphylococci. It is perhaps impossible to sterilize such hands. The wearing of rubber gloves, which are sterilized by boiling, absolutely excludes hand infection. The writer was led to wear gloves when

[9] Dr. James F. Mitchell (now in Washington, D. C.), at that time anesthetist in Dr. Halsted's operating room, took a photograph of the first surgical operation for which the operator wore rubber gloves (1893). This photograph he reproduced in his excellent article entitled "The introduction of rubber gloves for use in surgical operations." (*Ann. Surg.*, 1945, *122*, 902-04). Dr. Mitchell has been so kind as to send me the negatives of this as well as the other photograph taken by him at the same time. I wish to express my deep appreciation for his generosity.

[10] In the photograph, from left to right, according to Dr. Mitchell: Chauncey Pelton Smith, James F. Mitchell, Joseph Colt Bloodgood, Harold C. Parsons, John (orderly), Sidney Cone.

[11] Blumer, George. Reminiscences of an old-time doctor. *Yale J. Biol. Med.*, 1955, *28*, 9.

[12] Bloodgood, J. C. Operations on 459 cases of hernia in the Johns Hopkins Hospital from June, 1889, to January, 1899. The special consideration of 268 cases operated on by the Halsted method, and the transplantation of the rectus muscle in certain cases of inguinal hernia in which the conjoined tendon is obliterated. *J. Hopk. Hosp. Rep.*, 1899, *7*, 223-562.

[13] *Ibid.*, pp. 304-6.

[14] *Ibid.*, p. 292, footnote.

he operated because as resident surgeon he assisted Prof. Halsted at all of his operations, and was furthermore compelled to handle all sorts of infected cases, to make rectal examinations, and to operate on badly infected cases. He could not feel justified to operate without this protection. . . . The wearing of gloves practically excludes the danger of hand infection and leaves only one likely source of infection during operation—the skin of the patient. One can school himself to use gloves in almost any operation, and after a time forgets that he is using them.[15]

Dr. Mitchell introduced the method of anointing the hands with a sterilized boric ointment before the gloves are pulled on; it has proved most helpful. The gloves slip on more easily and are less likely to tear.

Observations on the impressive reduction of suppuration after introduction of rubber gloves conclusively proved their importance for the operating surgeon. Yet Halsted, who was responsible for this valuable innovation, remarks with admirable humility and frankness:

Thus the operating in gloves was an evolution rather than an inspiration or happy thought, and it is remarkable that during the four or five years when as operator I wore them only occasionally, we could have been so blind as not to have perceived the necessity for wearing them invariably at the operating-table. It is also noteworthy that none of the many surgeons, foreign and American, who visited our clinic in those years should have recognized the desirability of eliminating the hands as a source of infection, by the wearing of gloves.[16]

[15] *Ibid.*, pp. 304-5.
[16] *J. Amer. med. Ass.*, 1913, *60*, 1124.

The Squibb Laboratory in 1863

Edited by
GEORGE WINSTON SMITH*

The year 1958 marks the centennial of the founding of Edward Robinson Squibb's chemical and pharmaceutical laboratory in Brooklyn. As this anniversary approaches, Squibb holds a place of ever greater significance in the history of American pharmacy. His crusade for pure drugs, especially for the effective inspection of imported raw drugs, has become legendary. His scientific research, contributed with the highest ethical considerations to his colleagues and his government, helped to preserve his profession in the 1860's and firmly to establish pharmacy's respectable and independent status. Finally, as an imaginative manufacturer of pharmaceuticals, Squibb contributed to the diversion of pharmacy from mortar and pestle to machine production.[1]

The document presented here, a description of Squibb's laboratory written during March 1863 in the midst of Civil War when Squibb was turning out medicines for the Union army, is an account which, in its mention of Squibb's steam process for ether manufacture, its notice of the mechanical apparatus for the mixing of Blue Mass, and in other details, reveals the beginnings of the industrial revolution in pharmaceutical manufacturing. The presence, however, of fewer than fifty employees in the Squibb factory of 1863, together with various operations of hand labor such as rolling pills, labelling, filling bottles, and other slow processes, all indicate the relative infancy of the drug industry.

This description of Squibb's manufacturing laboratory is a report which was submitted by an able young Assistant Surgeon of the United States Army, Dr. Joseph Howland Bill,[2] to his superior officer, the Surgeon

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1 *The journal of E. R. Squibb, M.D.* (2 vols., Privately printed, E. R. Squibb and Sons, 1930); Edward H. Squibb to Charles F. Chandler, 14 March 1911, Squibb MSS., Squibb Institute for Medical Research Library, New Brunswick, N. J.; *Amer. J. Pharm.*, 1900, 599-603.

2 Dr. Joseph Howland Bill, a Princeton graduate of 1855, studied medicine in the office of his distinguished uncle, Dr. John Kearsley Mitchell, a professor in Jefferson Medical College, Philadelphia. Bill also attended lectures at Jefferson, and gained clinical experience with the help of his cousin, Dr. Silas Weir Mitchell, in the wards of Pennsylvania Hospital. Professor J. K. Mitchell characterized his nephew as "an industrious student, of irreproachable character . . . unusually accomplished in chemistry, physics, mineralogy, geology, etc." The last-mentioned scientific interests were developed when Bill served as an aide to the State Chemist and Mineralogist of New Jersey during a mineralogical survey of the State, and later in exploring coal and iron lands in Pennsylvania. In April 1858, Bill appeared before an army medical examining board and passed the examinations for appointment to the army medical department. He began his medical career in the army as a contract surgeon, but received an assistant surgeon's commission in 1860. He was present at the Battle of Valverde, where, his commander noted, "Assistant Surgeon Bill in charge of the ambulances in the field was distinguished for his energy and admirable arrangements for the relief of the dying and care of the wounded." In August, 1862, he was ordered to administer an emergency hospital for officers in Washington; three months later he became director of Hospital No. 3 at Frederick, Mary-

General of the United States Army, Brigadier-General William A. Hammond. It was the result of an order from Hammond to inspect the Squibb laboratory and give an account of its operations.[3] Hammond had conceived a project to establish government-operated laboratories for the manufacture of pharmaceuticals for the army virtually in competition with private enterprise.[4] Against the advice of Surgeon Richard S. Satterlee, Medical Purveyor in New York City,[5] and Edward R. Squibb, with whom he conferred on 31 January 1863, Hammond was determined to push ahead with his plan, and through the Quartermaster General's Office leased buildings at Astoria, Long Island, for a laboratory.[6] With government rivalry in drug manufacturing imminent, Squibb privately admitted in a letter to a friend that "Othello's occupation is (nearly) gone."[7] But it was entirely characteristic of Squibb that he should write to Hammond and offer "for the purpose of facilitating a sound judgment upon the project before it be undertaken, a free access to all the details and operations of my laboratory, to be used in your own way and for whatever length of time you may desire." He further suggested:[8]

In order to arrive at a good basis for your final decision I would suggest that one or more sound and judicious medical officers be ordered for a month or more, to inspect closely the entire operations of my laboratory including the purchases, sources of supply, prices and stocks of supplies necessary to be kept on hand; the processes of manufacture, putting up and packing; and the necessary qualities and qualifications in the way of buildings and apparatus and their cost. That this may be done as effectually as possible, I will expose to inspection all my bills, accounts and transactions freely, and will endeavour to place the officers you may send in such a position as may prevent their duty from being disagreeable to them. To obtain the best practical result they should be in the laboratory every day during the working hours, and keep notes which may be summed up at the close of their observations, and embraced in a concise report.

Because Squibb had a low opinion of Surgeon Charles McCormick,[9] designated by Hammond to be director of the new United States Labora-

land. Further orders, in January 1863, gave him supervision of the general hospital at Cumberland, Maryland, but these were cancelled, 11 February, when Surgeon General William A. Hammond selected him for duty in New York City. See J. H. Bill file. Personal Papers, Medical Officers and Physicians, Adjutant General's Office, War Department, National Archives, Record Group 94.

3 J. R. Smith, Surgeon U. S. Army, By Order of the Surgeon General, to Surgeon Richard S. Satterlee, Medical Purveyor, New York City, 24 February 1863 (fair copy), Surgeon General's Letterbooks, Military Letters, National Archives, R.G. 112.

4 Hammond first mentioned this project in his annual report, dated 10 November 1862. See Hammond to Edwin M. Stanton, Secretary of War, 10 November 1862 (fair copy), Surgeon General's Report Books, National Archives, R.G. 112.

5 For Satterlee's opposition to the proposed laboratory, see Satterlee to Hammond, 14 March 1863, Surgeon General's Office, Letters Received, National Archives, R.G. 112.

6 McCormick to Hammond, 19 January 1863 (telegram), Bill to Joseph K. Barnes, Acting Surgeon General, 26 February 1864, Surgeon General's Office, Letters Received.

7 Squibb to Dr. T. G. Catlin, St. Louis, 16 February 1863 (photostat), Squibb MSS., Squibb Institute For Medical Research, New Brunswick, N. J.

8 Squibb to Hammond, 2 February 1863, Surgeon General's Office, Letters Received.

9 Squibb to Catlin, 16 February 1863.

tory at Astoria,[10] and because McCormick had criticized the quality of Squibb's opium,[11] Hammond chose Assistant Surgeon Bill to visit Squibb's establishment. Bill remained there through April, and although specific reports which he intended to write on the various operations unfortunately were never submitted, his general or preliminary report was completed in the form that is presented here. In a covering despatch he assured Hammond: "I have already availed myself of all the opportunities afforded me for obtaining information on the manufacture of chemicals and have devoted the remainder of my time to a special study of the literature of the subject."[12] It must be obvious to any reader of Bill's report that Squibb must have prompted the young medical officer with facts which would prove, first, that the Squibb manufacturing laboratory with its facilities much less than completely in use was capable of sufficient expansion in production to provide the government with an abundance of high quality medicines, without any necessity for Hammond's experiment in state socialism, and, second, that the projected government enterprise would be unduly expensive from the standpoint of machinery costs and other outlays.

McCormick failed to put his laboratory into operation during the spring of 1863, but after his dismissal in May, Dr. Bill succeeded him as director and, perhaps aided by his experiences in the Squibb laboratory, soon organized a productive manufacturing depot at Astoria which sent out a variety of medicines to the army[13] until fire destroyed most of its apparatus in February, 1865, and consequently curtailed its operations.[14]

"PRELIMINARY REPORT ON THE ARRANGEMENTS AND OPERATIONS OF DOCTOR E. R. SQUIBB'S LABORATORY"[15]

Dr. Joseph H. Bill, Assistant Surgeon U.S. Army, New York City 9 March 1863

The Laboratory of Doctor E. R. Squibb is located in Brooklyn, L. I. about two hundred yards from the Fulton Ferry. It is built on a lot of ground lying between Doughty and Vine Sts. eighty feet deep and ninety-seven feet long. The building occu-

[10] Surgeon Charles McCormick, U. S. Army, had, in January 1863, just returned from New Orleans, where as Major-General Benjamin F. Butler's chief medical officer he had organized hospitals, enforced sanitary regulations, and carried out other duties in the occupied city. See *Butler's book. Autobiography and personal reminiscences of Major-General Benj. F. Butler* (Boston, A.M. Thayer & Co., 1892), pp. 403, 895.

[11] On the opium incident, see Squibb to Hammond, 28 January, 7 May 1863; Squibb to McCormick, 13 January 1863; McCormick to Hammond, 3 February 1863, all in Surgeon General's Office, Letters Received. Also J. R. Smith, By Order of the Surgeon General, to Squibb, 8 May 1863 (fair copy), Surgeon General's Letterbooks, Military Letters.

[12] Bill to Hammond, 30 March 1863, Surgeon General's Office, Letters Received.

[13] On the operations of this laboratory, see, Bill to Barnes, 26 February 1864; Bill to Satterlee, 31 December 1864, both in Surgeon General's Office, Letters Received.

[14] *Amer. J. Pharm.*, 1865, 234.

[15] Surgeon General's Office, Letters Received, National Archives, Record Group 112. Punctuation has been added wherever needed to facilitate the reading of this document. Irregularities of spelling and grammar have been preserved without repetitious use of *sic*.

pies three sides of a square and is four stories in height besides a deep basement. It is fire proof throughout; its walls are of brick; its ceilings of brick and iron arches. It is heated by steam pipes which are supplied by the waste steam from the engine; it is well lighted and ventilated, thoroughly furnished and abundantly supplied with water and gass.

The four stories into which the building is divided are again subdivided into rooms varying in size and suited in their several capacities for the divers work of a manufacturing laboratory.

To be more precise, then, the basement contains an engine room, a boiler room (in a vault entirely detached from the building to avoid dangers to this from explosions), a mill room used for powdering drugs and for the manufacture of Mercurial Pills etc., a bolting room in which the powders are sifted, a grinding room in which articles are reduced to coarse powders for the percolation process, a large cellar for the storage of Ether and Hyponitrous Ether and finally a large stable—eight rooms in all.

On the first floor are two offices containing a good fire proof safe, a large store room for glass ware, a smaller store room for crude drugs, a delivery room in which several boxes composing an invoice can be collected and inspected prior to delivery, a room not yet fitted up but suited for a vacuum pan for preparing fluid extracts and a mechanic's shop for repairs, etc.

On the second floor are a tinner's shop for the fabrication of tin bottles used for Ether, Chloroform, etc., a dressing room and water closet for the girls employed about the establishment, a room for pill making, bandage rolling, and labelling the bottles, a large miscellaneous store room with extensive closets for the storage of the products of the laboratory in bottles or packages, a packing room and a watchman's room.

On the third floor there is an experiment room, a study or library, a bottling room, three drying rooms, each capable of being heated to 150° F, and a stock room for the preservation in bulk of those products of the laboratory which must be kept on hand in large quantities.

The fourth floor is divided into an experiment room, a pharmaceutical room for the fabrication of compound pill masses or of those articles of the pharmacopoeia which are made simply by mixing their ingredients, a chemical room containing eight large copper evaporating pots convertible at pleasure into stills, besides an ammonia water apparatus, a heavy oil of wine apparatus, etc.,

etc., and a small room adjoining containing sand baths and naked fires.

In the middle of the building is placed the elevator, capable of raising to the fourth story a load of several tons.

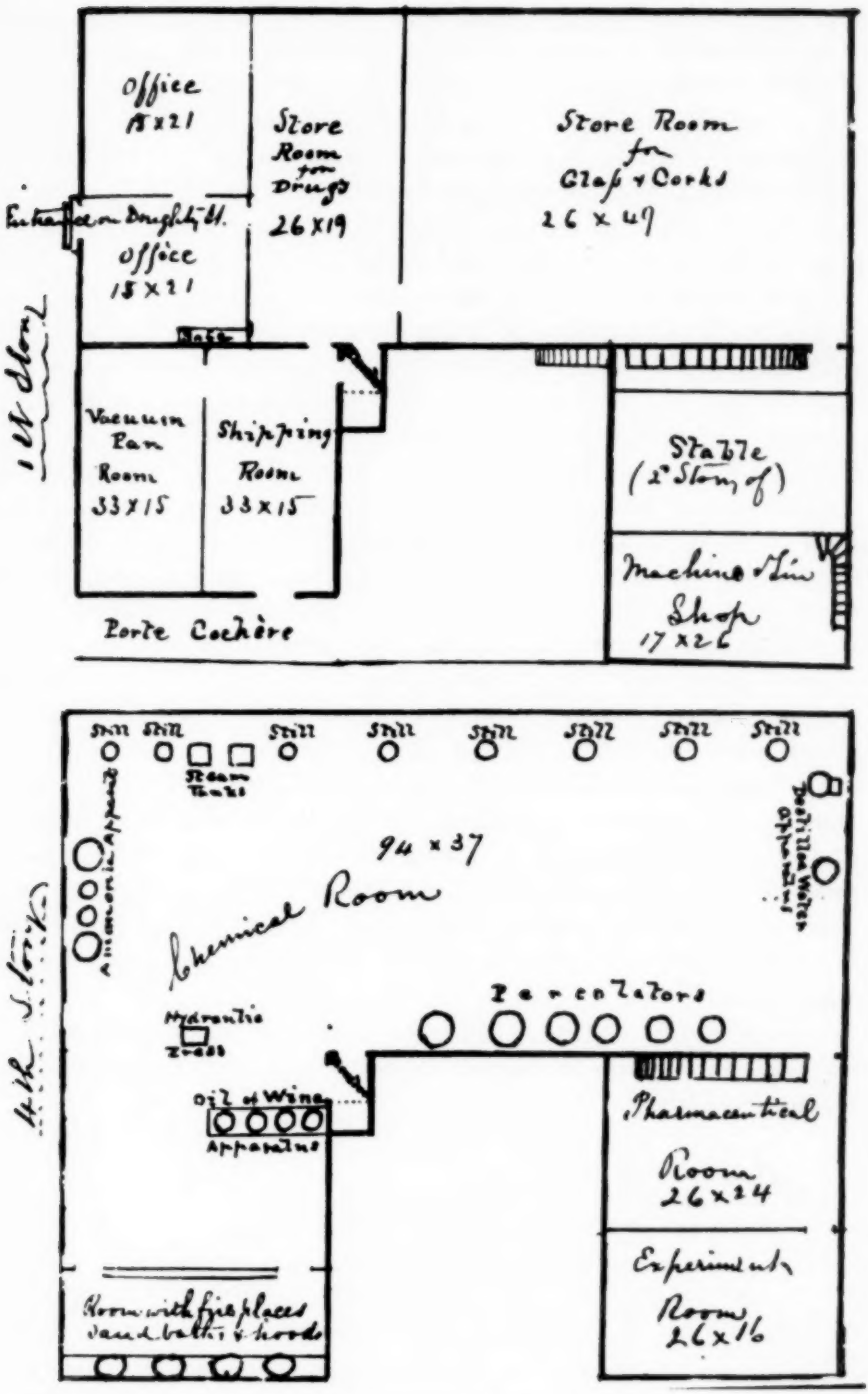
Entrance to the yard (which is enclosed on three sides) is gained from Doughty St. by means of a porte cochère.

I propose to describe the laboratory further by looking at it as divided into several departments.

First then, the executive department, Dr. Squibb being, of course, its head. This department makes all the purchases and sales, keeps the books, takes care of the correspondence, in a word is the business department of the laboratory. One clerk, one watchman, one messenger, and one errand boy here find employment. Besides the usual books of a business concern, there are kept books of processes, records of experiments, and books in which are recorded the monthly accounts of those with whom the laboratory has had dealings. The operations of this department are often intricate and puzzling to one unused to business forms and transactions. . . . The cost of this department, exclusive of a horse and wagon, is about twenty-eight dollars ($28) per week without any regard to the amount of work the laboratory may be doing. The average wages of its employees is six dollars and fifty cents . . per hand.

The department of Investigation and Study comes next. Three rooms, one on the fourth and two on the third floor, are devoted to the objects of this department. These rooms are fitted up as should be any analytical laboratory: the apparatus and books being all that is desirable. The cost of this department cannot be fixed but it is that of an analytical laboratory, depending upon the nature of the investigations. This department is a sine qua non of the laboratory. It is to this latter what the head is to the body corporate. It is quite as extensive and complete as is necessary even for a laboratory on a very much larger scale than Doctor Squibb's. It is this department which examines and pronounces upon every drug of doubtful honesty *before purchase*.[16] Here the refuse products of operations are examined to ascertain the thoroughness of the process employed, here the products of manufacture may be tested before bottling and sealing, here new processes must be devised, or old ones improved and modified. Doctor

[16] Information gleaned by Squibb in these laboratory tests of drugs was made available to the profession during 1863 in his famous "Report on the drug market," *Amer. Pharm. Ass. Proc.*, 1863, 175-95.



FIGS. 1 AND 2. Two of the diagrams drawn by Dr. Joseph H. Bill to accompany his "Preliminary Report."

Squibb and his chief assistant, Mr. Maisch,[17] manage these affairs. I do not think it necessary to fix the cost of the apparatus and fixtures of this department, but I *estimate* that it has cost already from one thousand to twelve hundred dollars. It would cost at the present high rates of exchange about fifteen hundred dollars. I may remark that the strictest economy and great judgement has been exercised in its outfit, and I do not believe it would be efficient if less had been expended upon it.

The department of applied chemistry and pharmacy is next in order. . . . It employs at the present time six male workmen at average wages of eight dollars and fifty cents per week or a total expense per [*sic*] wages per week of seventy-seven dollars.[18] I have as yet been unable to determine the producing power of the force. Dr. Squibb, however, informs me that his laboratory has furnished one twelfth of the supplies consumed by the army, that is of those supplies which his laboratory furnishes, besides doing a great deal of other work. This is, however, with a working force of from five to six times below the capacities of the building. I would fix the amount of supplies which this department *with its present force* can furnish, including everything on the Materia Medica list of the Army Supply Table—*excepting* those articles which for technical reasons it is expedient to buy directly from those who have manufactories especially adapted for their manufacture, e.g. Calomel, Adhesive plaster, Wine Magnesia, Sulphate of Quinia (perhaps)—at a sufficiency for one hundred thousand men. I shall, however, keep an account of the amount of medicines manufactured during this month and at the end of this time I hope to be able to state exactly the producing power of this department.

Seven rooms of this building are devoted to this department. On the fourth story is the laboratory for general purposes. Here Doctor Squibb has made

Arsenious Acid
Phosphoric Acid
Aromatic Sulphuric Acid
Heavy oil of wine (from Hoffman's Anodyne)

17 John Michael Maisch, the great German-American pharmaceutical chemist, left Squibb's laboratory about the middle of March 1863, to organize with Surgeon Andrew K. Smith the second of Hammond's laboratories, in Philadelphia. See John M. Maisch, "Statistics of the U. S. Army Laboratory at Philadelphia," *Amer. Pharm. Ass. Proc.*, 1866, 273.

18 The spring of 1863 was a period of unusually full employment in the Squibb factory. In contrast, there were but twenty-one persons on the payroll for the week ending 21 October 1863, and twenty-nine were employed in the week ending 11 March 1865. The wage rates increased during 1864. See E. R. Squibb "Time Books" in Squibb MSS., The Squibb Institute For Medical Research Library, New Brunswick, N. J.

Alum (recrystallization and granulation of)
Ammonia water
Ammonium chloride (recrystallization and granulation of)
Aromatic spirits of ammonia
Tartar Emetic
Fowler's Solution
Subnitrate of Bismuth
Bibron's antidote
Chloroform (purification of)
Materials for generating chlorine as a disinfectant
Collodion
Sulphate of Copper (crystallization of)
Extract of Belladonna
Extract of Colocynth compound
Muriated tincture of iron
Syrup of the Iodide of iron
Citrate of Iron and Quinine
Monsell's salt and solutions
Sulphate of iron (recrystallization of)
Oxide of iron (for antidote to Arsenic)
Yellow Iodide of Mercury
Sulphate of Morphia
Acetate of Lead (recrystallization of)
Acetate of Potash (recrystallization of)
Chlorate of Potash (recrystallization of)
Strychnia
The several salts and preparations of Zinc

In this room there are six kettles of copper provided with false bottoms for heating by steam and furnished each with a still head by which they can be readily converted into retorts. The chloroform still and the still for Aromatic Spirits of Ammonia are also in this room. These eight stills can readily distil fifty gallons of Alcohol (from percolating and extracting processes) a piece per diem, and other liquors in proportion. They cost of an average one hundred dollars each *before* the rise in exchange. In this room are two tanks each being double and heavily lined with block tin. Steam can be admitted between the inner and outer walls of these vessels and water to the amount of their capacities, eighty gallons, can readily be boiled in them. They serve to effect solutions of salts to be recrystallized, to wash precipitates by decantation, etc. Their cost was two hundred and fifty-seven dollars the pair. . . . The apparatus for making distilled water is automatic. The retort is of iron boiler plate; heat is applied by coils of pipe and steam, and the distilled steam is washed something on Soubeiran's plan. It cost one hundred and fifty dollars. There are here six large percolators of copper heavily tinned. These range from fifty to one hundred and fifty gallons capacity. All this ap-

paratus is not only expensive in the first cost but is also a source of continued outlay for repairs, more particularly for tinning which Dr. Squibb informs me requires to be frequently repeated. The percolators, I should have stated, cost about seventy-five dollars a piece. The Ammonia apparatus consists of a large retort of boiler iron and a succession of washing apparatus. The cost of this was about fifty dollars. It can furnish almost any amount of Ammonia water that can be required. There is in this room a brick furnace with four fires and four cast iron pots used as sand baths. In these pots on the sand are placed the very large glass retorts used in the process for making heavy oil of wine.[19] By an admirable arrangement all the gasses generated during this process are sucked out of the receivers in which they collect and driven into the tall chimney. In this way the laboratory (which would otherwise be uninhabitable) is kept free from sulphurous acid vapors. The yield of heavy oil of wine is very small, the danger and damage is often great and the whole process attended with trouble and anxiety. About one pound of heavy oil per day on a proportional number of pounds of Hoffman's Anodyne can be made by the four retorts. The cost of this apparatus was one hundred and fifty dollars. The retorts cost from seven to ten dollars apiece and are frequently broken. Finally in the loft which is a part of this room there is an excellent assortment of iron pots and kettles lined with porcelain, basins, dishes, of iron, porcelain and tin, glassware, etc., etc. stock in fact the value of perhaps a thousand dollars. Nitrate of Silver is made in a small room partitioned off from the main one, furnished with stone tables. Doctor Squibb prepares in these two rooms Syrup of Squills, most of the fluid extracts, Chromic acid and quite a number of other articles which he has not furnished to the Army. . . .

Ether and Hyponitrous ether are made in the basement in a room absolutely fire proof. The Ether condenser and washer is on Soubeiran's [word illegible] plan and furnishes its ether quite pure.[20] The 'continuous process' is employed, the Alcohol being

19 Squibb's formula for heavy oil of wine (oleum ethereum) may be found in his "Laboratory Notebook, No. 6 (1859-1861)," Squibb MSS.

20 A committee of the Boston Society for Medical Improvement made a study of the quality of ether commercially produced in 1861 and noted that two brands of ether were in common use, those of Powers and Weightman of Philadelphia and Squibb's. After asserting that both were "of excellent quality," the committee compared the two products and concluded: "The latter (Squibb's) is remarkably anhydrous, but possesses an odor harsh . . . disagreeable, and intensely etherlike . . . and in the opinion of those who have used it extensively, produces more choking during inhalation." See "Report of a Committee of the Boston Society For Medical Improvement, on the alleged dangers which accompany the inhalation of the vapor of sulphuric ether," *Boston med. surg. J.*, 1861, *65*, 230.

fed in under pressure from the story above. About two hundred lbs. per day of ether can be produced by this apparatus. I may state further that it is made of lead as nearly chemically pure as possible, is headed by coils of lead pipe; its joints are all 'burnt,' not soldered, and its cost as near as I can learn was two thousand dollars; this includes the condenser for the Hyponitrous Ether also which is a part of the Ether apparatus. . . .

To this department belong the drying rooms. These are three rooms on the third floor. Around the walls of these rooms are arranged frames for supporting the trays of wood, tin, glass and canvass which support the substances to be dried. Some of these trays are made of hard wood and plate glass and with great nicety. They are used for drying Citrate of Iron and Quinia. If made unevenly they would allow the solution of Citrate of Iron and Quinia to run into a thick mass which could not be dried. These rooms are capable of being heated without reference to one another from 60° F to 150° F. One of them is dark and two are lighted. The capacities of these rooms have always been far beyond the requirements of the laboratory. These rooms adjoin the bottling room and articles when dried can at once be taken into the bottling room and bottled. Another convenience of this arrangement is the facility it affords for drying the bottles and corks previous to filling. For drying opium previous to powdering and in fact for the drying of other drugs before subjecting them to this process, a small room in the basement close to the powdering mill is employed, heated and furnished like those above described. The time required for drying varies greatly. . . .

On the fourth floor is a room devoted to compounding of medicines, etc. such as pill masses, compound tinctures and solutions, etc. One person only is employed at this business. His wages are eight dollars per week. There is room in this apartment for five more workmen. The pill masses when thoroughly mixed are taken to the second story and placed in the hands of females. Six girls at present are so employed. They make about five thousand pills apiece per day. In the same room are four large tables. On these six girls and one man label and wrap bottles after they have been filled and corked. There are in all twelve women employed at an average wages of three dollars and fifty cents apiece. The man to whom I alluded is a supernumerary and not included among the six workmen of this department, and occasionally too, extra hands are employed as their services are

demanded. Work hours are from seven to six o'clock but occasionally a process which cannot be brought to a speedy conclusion will detain workmen until ten o'clock. I would fix the total cost of this department exclusive of tables, closets, etc. at five thousand dollars, but it must be recollected that this was before the rise in exchange. No platinum, silver or gold vessels are used in this department and in every respect the strictest economy consistent with efficiency is noticeable. . . . The bottling department comes next. All the products of the laboratory are here put up. Four men at eight dollars and fifty cents each per week are here constantly employed. One long room furnished with tables and closets is required for this work although the apartment actually in use would admit of an increase in the working force to twelve hands. In bottling there are several elements of expense. The cost of the bottles and corks is one. Doctor Squibb is compelled to keep constantly on hand upwards of two thousand dollars worth of bottles and corks. The loss of substance is a very considerable one. For instance, I saw eighteen lbs. of Creosote transferred from the original German package[21] to a number of two ounces (sic) vials such as are used in the medicine chests. The workman was doing his part of filling a large order of medicine chests now in Doctor Squibb's hands. The eighteen pounds served only to fill one hundred and twenty odd bottles and as each of these bottles passed for two ounces of creosote the whole hundred and twenty would pass for fifteen pounds. Here was a loss of three pounds. This loss is due to adhesion to the bottles and to the quantity in excess on every two ounces required to make the scales show 'full weight.' The stock of bottles and corks is kept in a large room on the first floor. The working capacities of this department are equal to the task of bottling all that the manufacturing portions of the laboratory can make. There is abundance of room in the bottling department for eight of ten more hands, some of whom could perhaps be profitably selected from boys or females.

The packing department occupies two rooms, a packing room proper and a shipping or delivery room in which the invoices of goods may be assorted into piles and lots, and so mistakes or confusion prevented. Two hands are employed at wages of eleven dollars apiece. There is room, however, for fifteen more packers.

21 German creosote, prepared from coal tar and "much cheaper" than the English article obtained from wood tar, had won a dominant position in the American market. See Edward Parrish, *A treatise on pharmacy*, 3d ed. Philadelphia, Blanchard and Lea, 1864, p. 505.

The Mechanical Department is next in order. The engine used is of twenty-five horsepower. The boiler furnishes steam for the laboratory processes as well as for the engine; it is fed by a small Worthington pump which returns the tepid water from the steam pipes used for heating the building into the boiler. Three mills are used for powdering. The stones are arranged after the fashion of those of any ordinary powder mill, they are all granite and run on granite beds. Their producing power is greatly in excess of the wants of the laboratory. In the same room is Squibb's apparatus for making Blue Mass,[22] etc. . . . Two hands are employed here (besides a tinner who is constantly engaged in making tin bottles). Their wages are ten dollars and fifty cents apiece per week. The cost for coal, for oil, etc. is that of any ordinary engine of twenty-five horsepower. In an adjoining room the bolting and sifting apparatus is kept. The various classes of medicines have each a set of sieves reserved for them. Opium, Dover's Powders, Ipecac, etc. are bottled in one set. Colocynth extract, Jalap, Gambage, Scammany, etc. in another, and so with the others. In this room too is a rolling mill for rolling opium into fine sheets for drying and to thoroughly kneed (sic) it in the Morphia Processes. A small room is partitioned off from this room and contains a grinding mill of cast iron for coarse powdering.

I am not able to state the total cost of this department. The engine, however, cost three thousand dollars. The boilers, pumps, and tanks seven hundred and twenty dollars; the belting and shafting one hundred dollars, the blue mass apparatus *about* four hundred dollars, the mills *about* three hundred dollars, the elevator five hundred and seventy-five dollars, and the hydraulic press for expressing the last liquid in percolation operations three hundred and thirty-four dollars, making a grand total for this department five thousand seven hundred and twenty-nine dollars. Other expenses not taken into account would no doubt raise this—which is rather an estimate than a record of facts—one thousand dollars.

The department of Storage is next and last. To this purpose two large rooms are devoted, each furnished with closets which can be locked. Ether, Nitre, Hoffman's Anodyne are not kept in one of these rooms but in a capacious cellar the temperature of

22 Blue Mass for Pilulae Hydrargyri, commonly known as Blue Pills, contained mercury (one grain to three of the pilular mass), confection of roses, and powdered liquorice root. By 1865 the trituration was generally performed by steam-driven machinery. See John B. Biddle, *Materia medica* . . . (2d ed., Philadelphia, Lindsay and Blakiston, 1865), p. 261.

which scarcely varies the year round. This cellar is furnished with closets in which the above-named articles can be stored when bottled and has plenty of room besides for storing the same articles in large quantities in carboys. Ten thousand lbs. of Ether, five thousand of Nitre and two thousand of Hoffman's Anodyne is *about* the capacity for storage of this cellar.

On the third floor is a large room for the storage of such articles as require to be kept on hand in large quantities as stock. Such articles are preserved in carboys. This room will hold a very large amount of stock. There are supplies sufficient for one hundred thousand men for a year of those articles which are kept in stock. On the second floor a very large room furnished with deep closets is devoted to the storage of those articles which it is desirable to keep under lock and key and which are not very bulky; Nitrate of Silver, Strychnia, Veratria, etc. The capacities for storage of this room are great and not exhausted in the present arrangement of the closets. I think that if advantage was taken of all the storage room the building affords this might be placed at a sufficiency for five hundred thousand men for a year.

Throughout the whole laboratory there is room for a large increase of the working force. Doctor Squibb has employed twice the number of hands now on his pay roll and with an increase of from thirty to forty hands during the day, and by relieving these by a night force of about sixty hands, the laboratory with a very little addition to its apparatus could furnish supplies for a million of men. By 'supplies' I mean those articles which Doctor Squibb has heretofore furnished. If Doctor Squibb at any time chooses to manufacture Quinia,[23] Calomel, Magnesia, etc. there is adjoining his premises a large lot of which he is already part-owner which he could purchase and on it erect an addition to his building suitable for the manufacture of these articles. I may be allowed perhaps to express the opinion that if Doctor Squibb were required to furnish a greatly increased amount of supplies that it would be greatly to his advantage to employ two sets of hands (one by night and one by day) rather than to make additions to his laboratory unless he were certain that the demand would be permanent. . . .

23 Although Squibb did not lead in the manufacture of quinia, he developed and published a formula for a fluid extract of cinchona. *Amer. Pharm. Ass. Proc.*, 1863, 65; *Amer. J. Pharm.*, 1863, *35*, 230.

Notes and Events

Edited by Dorothy M. Schullian*

The Anatomical Sketches of Vitus Tritonius Athesinus and their Relationship to Vesalius's TABULAE ANATOMICAE

The first significant publication of Andreas Vesalius was a series of six large sheets of anatomical illustrations accompanied by a descriptive marginal text. These so-called *Tabulae anatomicae,* published in 1538, are introduced by a dedicatory preface to Narcissus Parthenopeus, imperial archiater, in which Vesalius explains the origin of the work. He was appointed to the medical faculty of the University of Padua in the fall of 1537, and his first obligation was a series of surgical lectures for which he prepared a large illustration of the veins, presumably the origin of *Tabula* II. The detail and quality of this drawing represented such a novel pedagogical device "that the professors and students of medicine urged me to make similar illustrations of the arteries and nerves." Vesalius acceded to this request in conjunction with the anatomical dissection required of his appointment and carried out in December 1537.

The new illustrations were apparently as much appreciated as the first one on the veins; and, as Vesalius writes in his preface, "since many have vainly sought to copy my work, I have entrusted these illustrations to the press." They were accompanied by three views of the skeleton drawn by Jan Stephan van Calcar. Nevertheless, the contents of the *Tabulae anatomicae* do not live up to Vesalius's prefatory remarks, since *Tabula* I, which portrays the liver as the source of blood and depicts the organs of generation, had not previously been mentioned, and there is no illustration of the nerves.

Why the illustration of the nerves was not included in the published *Tabulae* is not completely clear. That Vesalius did produce a drawing of the nerves is evident from two sources. Somehow either the original drawing or a copy of it came into the possession of one Aegidius Macrolius of Cologne who published a single sheet in 1539 containing the drawing and a brief statement in which he acknowledged his Vesalian source and added that the sketch which had come into his possession was "so neatly done that I believe the author desired it to be available to everyone."[1]

In a letter of 1542 addressed to Johannes Oporinus, printer of the *Fabrica,* Vesalius refers to this particular instance of plagiarism as taken

*Cleveland, Ohio.

1 Ludwig, Choulant. *History and bibliography of anatomic illustration,* translated and annotated by Mortimer Frank. Chicago, 1920, pp. 187-8.

from "a rough sketch which I had drawn, to which I added an index, for one or two friends who sought it from me while I was preparing [the *Tabulae*] for publication." It may be assumed that Vesalius made some revisions in his drawings before turning them over to the cutter of the blocks, and at that time decided either that the illustration ultimately published as *Tabula* I was, for some unmentioned reason, more desirable or that his knowledge of the nervous system or ability to portray it properly was too faulty. That he did make such a drawing of the nerves, not a rough figure sketched for a few friends but rather an illustration to accompany his anatomical lectures, seems likely on the basis of the illustration published by Macrolius, and also on that of a second source of information, the notebook of Vitus Tritonius Athesinus.

This student and friend of Vesalius not only attended the first anatomical dissection performed by the new Paduan teacher from 6 to 24 December 1537 and filled twenty pages of his notebook with notes on the dissection, but in addition made some rough sketches of the anatomist's illustrative drawings.[2] In these notes we read that "the chart which the surgeon [Vesalius] has shown us and which he will have printed" as an aid for the students, made it unnecessary for them to record certain parts of the demonstration. Hence in December 1537 Vesalius appears already to have had a pedagogical motive for publication aside from any later thought he may have had about protecting his illustrations from plagiarists. Further on in the notes is the remark that "it is to be hoped that we shall be aided by those illustrations which may perhaps be published, containing depictions of the nerves, veins and arteries of the whole body." Possibly Vesalius wavered from time to time in regard to his decision to publish—or was this a reference to the great future work which he appears already to have had in mind? However, there certainly was a drawing of the nerves, though not of the whole body as we find in the *Fabrica*. Judging from the crude outline made by Vitus, who was no draughtsman, and labeled "Description of the third pair of nerves," (Fig. 1), the drawing must have been the original of Macrolius's plagiarism and the forerunner of the later, more sophisticated, illustration to be found in the *Fabrica*.[3] Vesalius's deprecating remark about "a rough sketch which I had drawn . . . for one or two friends" seems likely to have been an effort to lessen the significance of the plagiarized version printed by Macrolius.

Of the three other sketches in the notebook, two are fairly obvious copies of details, one from *Tabula* II, depicting a portion of the venous system (Fig. 2), and the other from *Tabula* III (Fig. 3), displaying the heart and portions of the arteries. The fourth sketch (Fig. 4) presents very

[2] The notebook is MS. 11,195 in the Austrian National Library, Vienna, ff. 184r-204r. The pertinent portions of the text were published by M. Roth, *Andreas Vesalius Bruxellensis*, Berlin, 1892, pp. 454-7, who refers to the sketches but did not reproduce them.

[3] P. 319 [419]

Fig. 1.

FIG. 2.

Fig. 3.

Fig. 4.

crudely a matter which Vesalius must have discussed during the course of his anatomical demonstration but did not include in his illustrations, namely, "The relationship," as the sketch is labeled, "of the veins of the uterus and of the breasts" (*Venae uteri et mamillarum concursus*). This was the Galenical belief that the inferior epigastric veins arose from the uterus and that anastomosis between the internal mammary, or superior epigastric veins, and the inferior epigastric produced a physiological relationship between the uterus and breasts. According to this idea, in the later months of pregnancy that portion of the menstrual blood not required for the foetus was drawn to the breasts where it was elaborated into milk. The sketch therefore represents the letters M and S in the upper and lower portions respectively of *Tabula* II, "[The vein] to the anterior part of the chest and breast," i.e., internal mammary vein, and "[The vein] to the pubes and to the transverse muscles of the abdomen. A large part of this [vein] continuing to the recti muscles joins with the veins of the chest," i.e., the inferior epigastric vein. On the same *tabula* Vesalius remarked that "how these [epigastric] veins communicate with the veins of the chest cannot be depicted on the present illustration lest other matters be made obscure." Hence the sketch by Vitus is a complementary illustration such as Vesalius was to produce later in the *Fabrica*[4] and on the final page of illustrations in the *Epitome*.

CHARLES D. O'MALLEY

John Bird on "Rekets" (London, 1661)

A meeting of the Historical Section of the Royal Society of Medicine in London on 4 December 1957 was devoted to Daniel Whistler and provided the opportunity to draw attention to an early book on rickets which appears to have escaped the notice of historians of the disease. It was the third book on the subject by an Englishman and the fourth all told if we count Arnoldus Bootius's chapter *"De tabe pectorea"* in *Observationes medicae, de affectibus omissis* (Londini, Tho. Newcomb pro Tho. Whitaker, 1649). It followed Whistler's *De morbo puerili Anglorum, quem patrio idiomate indigenae vocant The Rickets* (Lugduni Batavorum, ex officina Wilh. Chr. Boxii, 1645) and Francis Glisson's *De rachitide, sive morbo puerili, qui vulgo "The Rickets" dicitur* (Londini, impensis Laurentii Sadler, & Roberti Beaumont, 1650), and it preceded by thirteen years John Mayow's *Tractatus quinque medico-physici quorum . . . ultimus de rhachitide* (Oxonii, e theatro Sheldoniano, 1674). We refer to John Bird's *Ostenta Carolina. Or the late calamities of England with the authors of them. The great happiness and happy government of K. Charles II ensuing, miraculously foreshewn by the finger of God in two wonderful diseases, the Rekets and Kings-evil . . .* , which was published at London in 1661 (see Fig. 1 next page).

[4] Unnumb. page, m 3[r]

OSTENTA CAROLINA.

OR THE

Late Calamities of *ENGLAND*

With the AUTHORS of them.

The Great HAPPINESS and Happy GOVERNMENT

OF

K. CHARLES II

ENSUING,

Miraculouſly FORESHEWN by the Finger of GOD in two wonderful Diſeaſes, the

REKETS

AND

KINGS-EVIL:

Wherein is alſo ſhewen and proved,

I. *That the* REKETS *after a while ſhall ſeize on no more Children, but quite VANISH through the MERCY of GOD, and by MEANS of K.* CHARLES II.

II. *That* K. CHARLES II *is the LAST of KINGS which ſhall ſo HEAL the* KINGS-EVIL.

Diſcovered by the Hand of the Lord upon his unworthy Servant, and his Majeſties Subject

JOHN BIRD

And by him made publike for the Glory of *GOD*, Honor of the KING, and comfort of the PEOPLE of God.

Joh. 16. 13. ——*and he will ſhew you things to come.*

London, Printed for *Fra. Sowle*, and are to be ſold by *Robert Harriſon* the next Shop to Creed Church in Leaden-Hall-Street. 1661.

FIG. 1. Title-page of John Bird's *Ostenta Carolina.*

The medical significance of this book may have been overlooked for a number of reasons. Firstly, the word "Rekets" does not appear until halfway down a crowded title-page after two full stops and two commas, and since catalogues of seventeenth century books are wont to give short titles only, it is usually entered as *Ostenta Carolina,* suggesting a political tract occasioned by the Restoration, which it partly is. Secondly, the unfamiliar spelling "Rekets" may have obscured the fact that it deals with rickets. Thirdly, of the four copies recorded in Wing (B2954), none is in a specifically medical library.

It is a quarto consisting of 4 preliminary leaves and 91 pages of text. A short "Epistle to the Reader" follows a lengthy dedication "To our Dread Soveraign Lord Charles II" which sums up the scope and intent of the work: "SIR, I Here humbly offer into your Royal Hands a Message sent from Heaven for your Majesties acceptance. Written it is in the mystical Characters of Two Diseases, and partly upon your Royall Hands, which now heal one of these Diseases, and shall do the other."

Bird's description of rickets, as we learn from pages 43 and 64, is based on Glisson's *"Learned Treatise De Rachitide"* and on *"Arnoldus Bootius in libello De Affectibus Omissis, in Cap. De Tabe Pectoreâ."* Whistler is not mentioned. Like Glisson, Bird appears not to have known of Whistler's thesis, perhaps because it was published at Leyden and so was either unknown or unavailable in London.

The book makes no advance in either clinical description or pathology, but the author is refreshingly sceptical of the many empirical treatments in vogue. For instance Glisson, the eminent Regius Professor of Physic at Cambridge, Fellow and subsequently President of the Royal College of Physicians of London, had devoted twelve chapters to treatment and included among his *"Specifical alterant Medicines"* the use of *"Earthworms,* the Livers of *Frogs* and yong *Ravens, Woodlice,* washt in white Wine, bak'd in an Oven, and beaten to powder, and such like things," while "The *Chirurgical* [remedies] commonly received and approved in this Affect, and famous above the rest," he added, "are chiefly two; *Scarification of the Ears,* and *little Fountains* or *Issues.* But our enquiry . . . shal be extended to many more, namely of Cuppin-Glasses, Leeches, Blisters, Ligatures, and Swathing-bands."[1] Bird, however, unqualified and unknown, wrote:

> I Shall not have any thing to do here with those various Remedies which have been invented against this disease [p. 43] . . . although it cannot be denied, that many children have recovered from the *Rekets* to a soundness in every part, yet is the conclusion false, To assert thereupon, That this Disease hath been so cured by Physical Medicines [p. 67]. . . . For we deceive our selves, while we think, whatsoever is applied to such

[1] *A treatise of the rickets: being a diseas common to children.* Published in Latin by Francis Glisson, George Bate, and Ahasuerus Regemorter. Translated into English by Phil. Armin. Enlarged [and] corrected by N. Culpeper. London, Peter Cole, 1651, pp. 341, 310.

children, in such a time in which it pleaseth the Lord to be in healing them, to be the very causes of their recovery. As it cometh to pass, that such medicines which are applied unto persons sick of *Agues* or *Toothe-ache,* although most of them are known to be of no force against those griefs at all, if in the use of such Applications, the Agues and Tooth-ache leave such Patients, which ever almost sooner or later go away of themselves, are famed for most approved Helps against the said Diseases. Whence ariseth such a multitude of False Remedies against those and other Griefs [p. 68].

Bird believed in only one remedy, the Royal Touch, and expressed his pious opinion that "God Almighty hath ordained our Lord King CHARLES II to take away the *Typical Rekets,* even the *Bodily Disease* it self" [p. 70]. Comparing its efficacy with Glisson's crude and unbridled empiricism one may be allowed to conclude that the children of the seventeenth century would not have been helped more by it, but they would certainly have been harmed much less. Although Bird could not explain the healing power of the Royal Touch, he believed it was a sign from heaven which would one day be understood, and said:

Some Arts have quite perished (if we may believe Authors) which no man yet hath regained, yet we must not therefore conclude, that the refinding of them is impossible; for it might rather have been thought so before their first invention. Shall we say, because the Natives of *America* are wholly ignorant of the Mathematiks, that therefore those Sciences are impossible to be learned by them? [p. 15].

In his attempt to link the presence of rickets in the nation with the absence of divine grace due to the deposition of the House of Stuart, he searched the Bills of Mortality for possible confirmation from the time when the disease first appeared:

But one thing more touching the *Rekets* must not be forgotte, that is to say, the *time* when that Disease first seized upon Children in this Kingdome, and had that name of *Rekets* given unto it. It was the *third year* of our Royall *Soveraigns Nativity* [1633], or the next unto it, when the weekly *Bill* of *Mortality* mentioned *one* to have been *buried* of the *Rekets* in the *Moneth* of *May,* and no mention thereof in any of the said Bils before that time. As I have found by search in the *Parish Clerks Register* kept in their Hall [p. 78].

This date for the first appearance of rickets is somewhat later than that suggested by Whistler,[2] who puts it "some twenty-six years ago," i.e., *circa* 1620, and by Glisson,[3] who says, "This Disease became first known (as neer as we could gather from the Relation of others after a sedulous enquiry) about thirty years since [i.e., *circa* 1620], in the Counties of *Dorset* and *Somerset,* lying in the western part of *England.*" But it agrees with that

[2] As translated in George Frederic Still, *The history of paediatrics.* London, Oxford University Press, 1931, p. 199.

[3] Glisson, *op. cit.,* pp. 3-4.

given by John Graunt,[4] who states that "of the rickets we find no mention among the casualties, until the year 1634."

Incidentally "Rekets" was not simply a variant spelling of rickets and an example of seventeenth century orthographical licence. On the contrary, Bird deliberately adopted if he did not actually coin this form in order to stress what he considered its derivation. Originally Whistler[5] had suggested two possible etymologies for "The Rickets," one that it was derived "from the surname of some quack who was the first to treat it," the other that it "was of rustic origin arising . . . in the county of Dorset, where those who are short of breath (a frequent symptom in . . . this disease) are said in the dialect of the district 'To Rucket.'" Glisson[6] gave a third suggestion, which is the one now generally accepted, that

> The most receaved and ordinary Name of this Disease is, The RICKETS: But who baptiz'd it, and upon what occasion, or for what reason, or whether by chance or advice it was so named, is very uncertain . . . the name *Rickets* may be readily deduced from the Greek word *Rachitis,* or Rachites; provided, That we will but allow that consideration of change, which in vulgar pronounciation usually happeneth to words transplanted from one Language to another.

For this reason John Bulwer[7] suggested that it be called "more properly the Rackets." Bird's derivation of "Rekets" sprang directly from his thesis that the disease was closely connected in origin with the commonwealth and in cure with royalty in the person of Charles II.

> Let us examine [he said] the Name of REKETS, and therein shall we finde the names of those men which have caused our miseries. The word *Regentes* is a Latine word signifying those that are *Rulers* and *Governors,* whether in the highest or in a lower degree indifferently, from *rego* to *rule;* from which Latine word *Regentes* is derived a French word, and English, given to such a Person which exerciseth Regal authority though not a King. . . . This is the thing; *Governors* taking upon them Regall authority, not being Kings, and under Kings, are meant by this word REKETS, with no greater alteration of letters, but the very same which hath been found in our English tongue elsewhere. For of *Regents* leaving out the letter (*n*) is made *Regets.* . . . Then in *Regets* the letter (*g*) is turned into (*k*) and of *Regets* is made REKETS; which change is also found in our *English* tongue. . . . Thus in the Name of this Disease are in the first place discovered those men who were the AUTHORS of our late CALAMITIES; for according to the Name of the Disease they were not Kings, but such as took upon them REGAL AUTHORITY. For the Disease is not exprest by a word of the singular number REKET, but plurally REKETS, since not one single person, but many were the causes of our miseries. And in

[4] "Natural and political observations on the bills of mortality: by Capt. John Graunt, F.R.S. reprinted from the sixth edition, in 1676," p. 14, in *A collection of the yearly bills of mortality, from 1657 to 1758 inclusive.* London, A. Millar, 1759.

[5] Still, *loc. cit.*

[6] Glisson, *op. cit.*, pp. 4, 6.

[7] *Anthropometamorphosis.* . . London, William Hunt, 1653, p. 332.

this Name the *Cause* is put for the effect, for that those men were the cause of our evils, the *evils* are called by the Names of such who were the *Causes* of them [pp. 52-4].

It is perhaps of interest that in the nineteenth century it was suggested[8] that rickets might "possibly be derived from the Dutch *ruggegraat,* or the German *rückgrath,* the spine. . . . Dr. [John Mason] Good thinks it probably derived from the verb *rucken,* to rock, reel." In this century the problem is still unsolved: the *O.E.D.* says simply, "Origin doubtful; taken by Whistler (1645) as a corruption of Gr[eek] ῥαχίτης or ῥαχῖτις"—which in fact was not Whistler's suggestion but Glisson's.

Unfortunately we know very little about the interesting author of this book. It is dated "From my Chamber in *Sion College* in *London.* Jan. 24. 1660[1]" (misprinted *Ston* College in Wing). No record of him remains at Sion College: in the seventeenth and eighteenth centuries residential chambers there were often occupied by laymen, and it is likely that had Bird been a clergyman he would have held some official position and his name would have been preserved. From his "Epistle to the Reader" it appears that in the Civil War he adhered to the Royalist cause, but he states that what he did was "altogether from the first to the last peaceably, which notwithstanding, the distinction spoil'd my aged Father for the same cause and myself of our goods and quiet habitation. We bare it patiently." He describes himself as being "in that small number which the Scripture so often cals *Just men.*" He also mentions two other books by himself, firstly "a Treatise written by me called ROMAVATUM, wherein the History of the *Religion, Policy,* and *Destruction* of the City of *Rome* is discovered to have been portended in the *Theology,* and *Fables* of the ancient Heathen *Greek* and *Latine, &c.* not quite finished, Next . . . *Annotations* upon *Ezekiel,* chap. 1. *&c.* now ready for the Press." None of these has survived, if they were ever printed, and we suspect that he did not long outlive the publication of what is now his only extant book. But of his piety he left his readers in no doubt, for he concluded his prefatory Epistle with the admonition, "Reader farewell, and let God have the praise of all which is true herein, as from whom it came; all the errors and imperfections I challenge as mine own, excepting some few *mistakes* of the Printers."

As Bird appears to have had some knowledge of and practice in medicine we searched the MS. Annals of the Royal College of Physicians, London, for possible mention of him and discovered that a person of the same name had presented himself for examination at the *Comitia* held on 6 November 1629: "Johannes Bird Baccalaureus Oxoniensis ex Collegio Mertonensi: comparuit et post breve colloquium rejectus est et admonitus

[8] "Select medical biography," p. 148, in *The cyclopaedia of practical medicine,* edited by John Forbes, Alexander Tweedie and John Conolly. London, Sherwood, Gilbert and Piper. . . ., vol. 4, 1835.

ut aliquot adhoc annos studio Medicinae impenderet."[9] Two years later on 14 October 1631 he appeared again before the College, where "accused to practize without license: hee confesseth hee hath practized three yeares in towne: since his last returne hether, and doth still practize: and hee knoweth not why hee should not practize. Dr. Foxe aduised him to bethincke himselfe of some cause to give the Colledge satisfaction." On 25 June 1632, in the presence of William Harvey *inter alios*, "Dr. Wright complayned of Mr. Bird for giving phisicke one friday the 20th of June to one Mrs. Collier a Brewers wife, which hee will prove," and on 3 July 1632 the Beadle was ordered to summon Dr. Wright to provide details of his charge. But no more is heard of the matter in the Annals, and one must presume that Bird left London and the area of the authority of the College of Physicians.

Anthony à Wood[10] has the following entry about him:

> John Bird of Merton coll. was admitted [Bachelor of Arts] the same Day [7 February 1624]. Whether he be the same with Joh. Bird, who was afterwards schoolmaster in the City of Glocester and Author of *Grounds of Grammar*, Oxon, 1639, oct. I cannot say to the contrary. He that was batch. of arts was an Oxfordshire man born, and had been originally of St. Edm. hall. All which, batchelors, except . . . Bird, will be mentioned in another part of this work.

Joseph Foster[11] added that he was a plebeian of St. Edmund Hall and matriculated 28 June 1620 aged 16. Richard Smyth[12] may have been referring to the same man when he noted that on 21 September 1665 "Dr. Bird (as commonly called), to whom I lent 20s. (a desperat debtar), [was] buried *ex peste*." Dr. Bird, "as commonly called" either because of his book on "Rekets and Kings-Evil" or his unlicensed medical practice, "a desperat debtar" perhaps because he had been "spoil'd" of his "goods and quiet habitation" during the Commonwealth, may have been buried *"ex peste"* because he tended sufferers from the plague until he fell a victim himself, thus ending his life in pursuit of that profession in which he had hoped to spend it.

ACKNOWLEDGMENTS

We thank Sir Harold Boldero, Registrar of the Royal College of Physicians of London, for permission to consult and quote from the MS Annals; Miss Elizabeth Edmonston, Librarian, Sion College, for kindly searching records; and the Photographic Department, The National Hospital, Queen Square, London, for the figure.

RICHARD A. HUNTER
IDA MACALPINE

[9] John Bird, a Bachelor of Arts of Oxford from Merton College, appeared and after a short viva was rejected and advised to devote himself for several more years to the study of medicine.

[10] *Athenae oxonienses*. . . A new ed., with additions, and a continuation by Philip Bliss. Vol. 5, *Fasti oxonienses*, Part 1, London, F. C. & J. Rivington, 1815, p. 411.

[11] *Alumni oxonienses: the members of the University of Oxford 1500-1714*. . . Oxford, Parker & Co., vol. 1, 1891, p. 127.

[12] *The obituary of Richard Smyth*. . ., being a catalogue of all such persons as he knew in their life: extending from A.D. 1627 to A.D. 1674, edited by Sir Henry Ellis. London, Camden Society Publication No. XLIV, 1849, p. 68.

The Reception of William Beaumont's Discovery
Some Comments on Dr. Poynter's Note

The following remarks have been evoked by a recent note in this *Journal* (1957, *12*, 511-12) by Dr. F. N. L. Poynter on the reception of William Beaumont's work in England. In it he refers to the English version of my doctoral dissertation which originally appeared twenty-three years ago in Germany (*Die Aufnahme der Entdeckung William Beaumont's durch die europäische Medizin. Ein Beitrag, zur Geschichte der Physiologie im 19. Jahrhundert.* Abhandlungen zur Geschichte der Medizin und der Naturwissenschaften, Heft 8, Berlin, E. Ebering, 1935). Naturally, I was both interested and gratified to see that the first product of my efforts in the history of medicine was still viable enough to arouse discussion and comment.

At the same time it was equally obvious to me that Dr. Poynter's evidence had small bearing on the main thesis of my monograph, namely, that there was a differential response to Beaumont's work in England, Germany, and France.

One of the most interesting results to emerge from our study [I had originally concluded], is an appreciation of the very characteristic manner in which these fundamental observations and discoveries were received and developed in the different countries. The significance of the man and his work was first recognized in Germany. Here interest centered chiefly upon the scientific problems arising from the first study of physiological processes in a living human being. Medical practitioners remained almost entirely uninfluenced by these researches. Indeed, we are far from surprised by this situation, for who would expect anything else in the period of Johannes Müller and the beginning "physiological" and "rational" medicine.

The English came somewhat later. It is not surprising to find that the first English writer to discuss Beaumont's work approached the problem from a clinical point of view, or that the later attempt to utilize Beaumont's results for prophylaxis and therapeusis was marked in England.

The French took notice of Beaumont's work latest, and for them his researches became a point of departure for investigations involving animal experimentation. These were inaugurated by Blandlot's artificial gastric fistula in animals.

When the English translation of this study was reviewed by Genevieve Miller (*Bull. Hist. Med.*, 1943, *13*, 111-13), she called attention to a number of references to Beaumont and his work in England and France in the later 'thirties and concluded that "in the light of this new evidence it would appear that there was actually very little difference between the times of first notice of Beaumont's book in the three countries [Germany, England, France]." She went on to say, furthermore, that "it is not clearly demonstrated that the various countries were essentially different in their reception." Now Dr. Poynter adds two more references (Mayo, 1837; Smith, 1836-37) to those adduced by Dr. Miller and concludes that "these addi-

tional English references to Beaumont seem to strengthen the doubts of the validity of Rosen's thesis expressed by Dr. Miller."

While Dr. Poynter is aware that I discussed Dr. Miller's conclusion in 1943 (*Bull. Hist. Med.*, 1943, *13*, 631-42), he has inadvertently overlooked data contained in that paper as well as the discussion of their significance. For this reason, the present communication will review the problem at issue and indicate the bearing on it of the facts mentioned by Dr. Poynter.

The concept "reception" involves two aspects that must be differentiated. These are:

1. The *time* aspect, that is, the temporal interval which elapses between the appearance of the results of a scientific investigation and the date when they first receive notice elsewhere; and

2. The aspect of *interaction,* that is, the reaction which these results elicit in the places where and among the groups to whom they become known. Interaction may manifest itself

(a) Qualitatively, by the kind of reaction evoked, or

(b) Quantitatively, by the degree or intensity of interaction which results.

We can now proceed to see how the facts presented by Dr. Poynter bear on these matters. In my reply to Dr. Miller, I not only acknowledged her correction of my statement that there was a difference between the times of first notice in Germany, England, and France of Beaumont's book, but added five additional references from the British literature between 1834-35 and 1838. Consequently, the point had already been established in 1943 by both Dr. Miller and me that Beaumont's work had been noticed at about the same time in England, France, and Germany. To this Dr. Poynter adds nothing beyond two references that strengthen the conclusion.

Does the evidence, however, justify the further conclusion that the reaction to Beaumont's work was not different either quantitatively or qualitatively in the countries concerned? The answer to this question, in my opinion, must still be an unqualified "No." I shall not repeat here the evidence adduced in my paper of 1943. The reader is referred to that for the full presentation. It can be said that there is nothing in Dr. Poynter's evidence to contradict the conclusions quoted above. To be sure, Herbert Mayo referred to Beaumont's work, but what of that? Did he do anything with this information? Was he stimulated to further investigation? And as for the reference to Southwood Smith, it means even less. All it shows is that Smith was up to date in his reading. The reference is contained in a work the purpose of which was health education, and this circumstance might even be used to support my contention that the English reaction is characterized in part by an "attempt to utilize Beaumont's results for prophylaxis and therapeusis." What after all is health education, if not an aspect of preventive medicine? Furthermore, it should be pointed out that

a number of the early reviews of Beaumont's work in the British literature were rather unfavorable. There is no evidence of which I am aware to show that Beaumont's work stimulated purely physiological research in Britain. On the other hand, this did happen in Germany and later in France.

The analysis need not be continued further. Any interested reader has been given the sources to which he can go for all the data involved in this discussion. What is more important, however, is that the problem of interaction to which attention has been called has not apparently attracted much interest. Neuburger dealt with British-German interactions, and there have been a few other studies of this type. Yet here is an area which if intensively studied would undoubtedly clarify greatly the dynamics of the transmission and spread of medical and scientific knowledge.

GEORGE ROSEN

New Light on the Reception of William Beaumont's Discovery in England

"The dynamics of the transmission and spread of medical knowledge"[1] is a subject which merits the attention of the historian at any time. The study of reciprocal influences has already proved its value in other fields of historical scholarship and it is important that the methods likely to give the most accurate results in the history of medicine should be developed and tested. So far, there are few studies of this kind which may serve as guides. If in an attempt to assess the degree of interest aroused in a particular region by a new discovery a quantitative analysis of references is employed, then it must take account of all the ascertainable facts. From the purely *quantitative* aspect it is a case of all or none, although we may also grade these references according to comparative importance on qualitative grounds.

Before I go on to state explicitly the qualitative factors involved in the references to Beaumont's work which the brevity of my earlier note may have left obscure, there are additional facts to report which affect still further the quantitative aspect. It would have been strange indeed if *The Lancet* had omitted all mention of Beaumont's work, and a reference in that journal (1835-6, *2*, 238-44, 14 May 1836) must now be added to the tally. The occasion for it, as for the notice in the *British and Foreign Medical Review* of the same year already cited by Rosen,[2] was the review of Andrew Combe's *Physiology of Digestion*. This consists of 11½ columns of close type of which only one column is devoted to Combe's book and the whole of the remainder to a summary of Beaumont's observations, which are introduced to the reader as "in many respects novel, and of more importance than anything else recently published." An editorial reply to

[1] Rosen, George, in "Notes on the reception and influence of William Beaumont's discovery," *Bull. Hist. Med.*, 1943, *13*, 642.

[2] Rosen, *op. cit.*, p. 633.

a query on digestion published on 10 June 1837 (1836-7, 2, 416) refers the correspondent back to this notice for "much valuable information on the subject."

Yet another reference is to be found in the notice given in the *Medico-Chirurgical Review* (1838, 29, 205-7) to the Edinburgh reprint of Beaumont's book. This begins by stating that

> Dr. Beaumont's experiments have excited great interest in every country of the civilized world, and Dr. Combe has conferred a benefit on the profession of this country by the present reprint, and by many valuable notes appended to it. It is a work which will not admit of analysis; but its size and price will render it easy of access to all who take interest in the subject, or indeed in their profession. But as Dr. Beaumont has drawn up a summary of the inferences to be drawn from his numerous experiments, these deserve as extensive a diffusion as a periodical can give them. These therefore we subjoin.

After listing the inferences, the anonymous reviewer comments that "a vast majority of these inferences were come to by observation and experience long before accident disclosed to Dr. Beaumont the secret operations of nature in the process of digestion." He goes on to question the truth of two of the inferences and to claim priority in the publication of a third.

Finally, there is a curious omission which has so far gone entirely unremarked. In October, 1837 there began in London the publication in parts of the English translation of Johannes Müller's *Handbuch der Physiologie*, and on 1 January 1838 Müller's own account of Beaumont's work became available to English readers. The translator was Dr. William Baly (M.D. Berlin, 1836), who was at the time physician to the St. Pancras Infirmary and who later was elected a Fellow of the Royal Society and of the Royal College of Physicians and appointed physician to the Queen before his early death in 1861 at the age of 47. This example of bilateral—or even trilateral—influence is surely worthy of notice in any study of interaction. The successful investigation of gastric juice by Müller and Theodor Schwann was already known to English readers through an account in the *British and Foreign Medical Review* (1837, 4, 201-5). It is of interest to find the English view of Müller given in a brief note on the appearance of Baly's translation in the *London Medical Gazette* (1837-8, *n.s.1*, 134).

> The name of Müller [it is stated], has for some years been known to the English reader with progressively increasing reputation. We have ourselves given many of his views and opinions to the world, and some of them coming direct from Berlin, with the advantage of his own revision. Müller is decidedly the great physiologist of the day; and his precise descriptions and methodical ratiocination leave the inconsequential vivisections of Magendie at an immeasurable distance.

To summarise the quantitative data as they are now known to us, it may be stated that within five years of the publication of Beaumont's book

there appeared in Britain an edition of the book itself and eleven other references to it, while in Germany there were a translation and seven other references. Moreover, it should be remembered when we consider the time lag between the German translation and the Edinburgh reprint that copies of the Boston reprint of 1834 (*vide* Mayo, 1837) could be obtained in England and studied without a language barrier. If these figures have any significance, they would seem to indicate a wider (but not therefore more intensive) interest in Britain than in Germany.

Turning now to the qualitative aspect of this reaction, we find that of the eleven references only three are unfavourable, the least favourable notice being motivated not by clinical or therapeutic prejudices but by rival physiological theories. In the general histories of medicine (and even in histories of physiology) it is customary to find the assertion that there was little or no physiological work done in Britain in the first half of the nineteenth century. Interest in a scientific problem cannot be judged only by success in solving it. While Germany was fortunate in having at that particular time one of the most gifted physiologists of the century, there were in Britain a number of men who were practising physicians and surgeons as well as teachers and experimental physiologists. The lack of academic organization in Britain should not mislead us into believing that there was little or no research. A broad survey of the medical literature published in Britain at the time indicates the contrary. In the peculiar and highly individualistic system characteristic of Britain at that time there were private, individually owned laboratories, just as there were private, individually owned medical schools. Benjamin Collins Brodie was a successful and fashionable surgeon, but he was also a talented physiologist who was awarded the Copley medal of the Royal Society for his researches and who had investigated the effects of curare on the nervous system as early as 1811. Both he and Herbert Mayo had made experimental studies of the process of digestion in dogs before Beaumont's book was published. William Prout's paper to the Royal Society in 1824, in which he identified free hydrochloric acid in the gastric juice, had also aroused considerable interest in the problems of digestion. Charles Bell, Mayo, and Marshall Hall had done excellent work on the nervous system. William Sharpey, only a year Müller's junior, had already begun his long and distinguished career and in 1836 became the first professor of physiology in England and the founder of an important English school of physiologists. There were a number of good textbooks of physiology which show that the subject was by no means neglected in medical training. Baly's translation of Müller's monumental *Handbuch* must have presupposed a body of readers sufficiently interested and well informed in physiology to make use of it. Baly was a successful practising physician, but he must also have been an enthusiastic physiologist to have undertaken this great task, and his editorial annotation shows that he was a good one. The appreciation which

Müller's work received in London underlines the fact that there was in Britain a substantial and well-informed body of medical men particularly suited by their interests and pursuits to seize upon the physiological significance of Beaumont's work. Does not consideration of this background imply that we should not accept too readily the suggestion that British interest in Beaumont's work was mainly clinical and therapeutic?

Finally, there are qualitative aspects of the two references given in my earlier note which deserve further comment. Both accounts are highly favourable. Mayo's was no mere casual reference but a substantial summary to which he drew special attention in the preface to his book. The fact that Mayo was himself an experimental physiologist and a teacher also has significance, and probably accounts for the fact that he was the first to incorporate Beaumont's work in an English textbook of physiology intended for students, and so had presumably already introduced it into his oral teaching on the subject. The time factor here is important, for his book was written and printed in 1836 and published on 1 January 1837. It has been thought worthy of note that Beaumont's observations were described in a German textbook of 1843.[3] What is, or was, the usual time lag between discovery and textbook presentation is a point I have not had the leisure to elucidate. If Mayo may be credited with the first textbook account of Beaumont's work, then Southwood Smith's presentation of it to a far wider public signifies more than that he was "up to date in his reading." His book was intended for the "intelligent artisans" who were then enrolled by the thousands in the Mechanics' Institutes. In tracing the spread and influence of medical knowledge, the point where a new discovery first becomes established in routine medical teaching and the point where it is made available to the general public are events which are of more than trifling significance for the medical historian.

F. N. L. POYNTER

3 Wagner, Rudolf. *Lehrbuch der speciellen Physiologie.* Leipzig, 1843. Reported in: George Rosen, *The reception of William Beaumont's discovery in Europe.* New York, Schuman, 1942. p. 51.

[*Editor's Note.* The editor now writes FINIS for these pages to the discussion on the reception of William Beaumont's discovery in England.]

Galen on Human Dissection

Can anyone tell me whether the following passage from Galen's *De compositione medicamentorum per genera* has ever been noted in the literature? In book III, chapter 2 (Kühn, *13,* 604) he says:

And so if in apes you frequently see the position and size of each tendon and nerve, you will remember exactly, even if you have the opportunity of dissecting a human body, how to find quickly each one as you have seen it. But if you are quite untrained, you will not get any benefit at all from such an opportunity; just as the physicians in the German war, who had permission to dissect the bodies of the barbarians, learned nothing more than what butchers know.

The implications of these remarks are obvious; taken together with the similar ones in book III, chapter 5 of his *De anatomicis administrationibus* (p. 77 of Dr. Charles Singer's translation, 1956) they seem to indicate clearly that there was at least sporadic, fortuitous dissection of the human body in the time of Galen and that the chances are good for his having had direct knowledge of its structure. I should be very glad to learn whether this passage has been taken into account in any discussions of the question.

MARGARET T. MAY

The Institute of the History of Medicine at the University of Bonn

The University of Bonn was founded in 1818 by King Frederick William III of Prussia. The Medical Faculty of this comparatively young university therefore reflects the developing new medicine and presents a true picture of medical progress in the nineteenth century. Lectures in the history of medicine were given from almost the earliest days, and the services in this field of men like K. J. H. Windischmann (1775-1839), Moritz Naumann (1798-1871), Oskar Witzel (1856-1925), and Hugo Schulz (1853-1932) laid the foundations for later work. From 1906 to 1942 Karl Schmiz lectured regularly, and on his death in 1946 his valuable reference library came by bequest to the new Institute of the History of Medicine.

The present Institute was founded in 1943 under the directorship of Professor Johannes Steudel, pupil of Walter von Brunn at the Sudhoff Institute in Leipzig. The development has been rapid. The special interests of Professor Steudel in anatomical nomenclature before Vesalius and in Paracelsus, Leibnitz, Goethe, and Schiller are supplemented by those of his first assistant, Dr. Gernot Rath, who works in the history of neuropathology, Avicenna, the history of the plague, pathology of the nineteenth century, and the history of the Medical Faculty at Bonn, and of his second assistant, Dr. Heinrich Schipperges, who is concerned in particular with mediaeval and Arabic medicine and with the scientific works of Hildegard of Bingen. Professor Steudel is president of the Deutsche Gesellschaft für Geschichte der Medizin, Naturwissenschaft, und Technik and one of the editors of the re-born *Sudhoffs Archiv für Geschichte der Medizin und der Naturwissenschaften.*

The heart of the Institute is the library, which lacks older original titles but offers excellent working facilities because of its great number of reference books, dictionaries of ancient and modern languages, manuals, books on the histories of European universities, biographies, philosophical works, etc. A suitable journal collection is maintained. A special room provides photographic services. The seminar room is adequate for about thirty students. Here are held the lectures, which are now compulsory, for medical and dental students. Starting with introductory sessions for beginners, the lectures proceed to a systematic survey from ancient to modern

times. Seminar courses during the winter deal with such subjects as the physician at the sickbed, the history of psychosomatic medicine, the period of Johannes Müller, and the era of Hippocrates. In the summer, courses on the history of balneology are offered; these are supplemented by visits to various spas. During both the winter and the summer terms selected Latin texts from medical authors of ancient and mediaeval times are read, and instruction is given on methods of historical research. In addition, excursions in medico-historical topography have been made to Copenhagen and London and through Franconia and Upper Italy; one to the South of France is scheduled. These excursions, in groups of about twenty students, serve to widen the general and scientific horizons of the participants and allow them to study more closely the historical and sociological backgrounds of foreign civilizations and institutions of medical interest.

It is hoped that a medico-historical museum can soon be added to the facilities of the Institute. A warm welcome is offered to visitors and guests from Germany and abroad. Sleeping accommodations can be provided for a few nights. A small kitchen offers physical restoration during working hours. Many a scientist from Germany and abroad has already accepted the hospitality of the Institute. Foreign postgraduate students on the Humboldt Fellowship are accorded special arrangements.

Theses from the Institute in past and current years include studies in anatomical nomenclature, the history of balneology, Galen's etiology of diseases, the development of modern Persian medicine, the history of bandages, and Arabic manuscripts. They are all part of the main objective of the Institute, which is to give the medical student a general academic preparation and education and a historical foundation of medicine. In this way the Institute hopes to serve as a balance wheel to the continuously growing tendency towards specialization in medical education and the consequent narrowing of interests and isolation of the single faculties of medicine. It hopes thereby to make of the history of medicine a connecting link between the faculties of medicine and of humanistic studies.

H. Schipperges

David A. Tucker, Jr., 1890-1958

At state, national, and international meetings for the history of medicine the quiet modesty of David A. Tucker, Jr., who died on 3 June, will be deeply missed. Faithful attendant over a long period of years at such meetings, loyal worker on committees, devoted supporter of efforts to include courses on the history of medicine in the curriculum of medical schools, he did his work steadfastly and unpretentiously and with a care which gave it lasting value. It was all done in the midst of an active practice in internal medicine and heavy academic responsibilities, and it belies the frequent declaration of busy physicians that they have no time for the history of medicine.

Dr. Tucker was graduated M.D. from the University of Cincinnati College of Medicine in 1916. His affection for that institution abided over a period of more than fifty years. He was president of the Cincinnati Academy of Medicine in 1945-46 and president of the Ohio Academy of Medical History in 1954-55. As a true Cincinnatian and ardent medical educator he abetted the growing esteem for the contributions of Daniel Drake. Much of the success of the Drake Centenary, which was held in Cincinnati on 27 May 1952, was due to his efforts, and in the same year he published through the Institute of the History of Medicine in Baltimore an edition of Drake's *Practical essays on medical education and the medical profession in the United States.*

On 26 April 1958 Dr. Tucker delivered before the Ohio Academy of Medical History his last paper, "The origin of medical education in the Ohio Valley." At that meeting historians were informed of plans for the David A. Tucker, Jr. Library of the History of Medicine. His library, conceived definitively when he read the first edition of Garrison's *Introduction to the history of medicine,* has gone quite naturally and fittingly to his alma mater, with the hope that "the books will be used for the purpose for which they were collected, namely, for the teaching of the history of medicine." The library consists of some 1,300 items; a list of about two hundred of them was issued in time for the meeting and can be procured on application to Joan Titley, Librarian, College of Medicine, University of Cincinnati. The library illustrates two verities that guided Dr. Tucker during all the years in which he collected it: "He who looks back to the old and knows the new is worthy to be a scholar," and "No one who lacks an understanding of the origin and development of medicine can claim to be a 'COMPLEAT PHYSICIAN.' "

Italian Broadsides at the New York Academy of Medicine

Through the generosity of Mr. Mortimer Neinken, the New York Academy of Medicine has recently received nearly two thousand Italian broadsides, most of which deal with quarantine and other measures for the control of communicable disease. A preliminary survey of the collection has been published in the *Academy Bookman* (1958, *11,* No. 1). The material includes 10 broadsides of the late sixteenth century, 870 of the seventeenth century, 1,041 of the eighteenth century, and 47 of the nineteenth century. The earliest and latest documents are dated 1576 and 1849 respectively. Most of the broadsides are clearly printed and well preserved. Many are ornamented with shields and other decorative devices of historical interest.

The collection is not only aesthetically attractive but provides also a rich hoard of primary source material on such subjects as: location and spread of certain communicable diseases; conceptions of cause, treatment, and prevention; structure and function of official health agencies; identity of numerous persons involved in preventive activities; social conditions; ju-

dicial and penal action; and the names of official printers. It is hoped that scholars, both medical and nonmedical, will feel tempted to study the Neinken broadsides. Such study is certain to prove rewarding.

SAUL JARCHO

George Sarton Memorial Foundation, Inc.

A communication from its president, Chauncey D. Leake, discloses that the suggestion of C. Doris Hellman Pepper for a George Sarton Memorial Foundation has now achieved realization. The suggestion was made at the Council Meeting of the History of Science Society on 20 October 1956. The general purpose of this nonprofit corporation is to promote the efforts of Dr. Sarton in the history and philosophy of science; its initial objectives are to aid in maintaining publication of *Isis* and to provide lectureships and fellowships to advance the study of the history of science.

May Sarton serves as vice-president of the Foundation, Morton Pepper as secretary, and William D. Stahlman as treasurer. The Foundation will work in close co-operation with the History of Science Society which created it and with the Society's president Henry Guerlac. Assistance is welcomed in promoting its objectives; contributions may be sent to Dr. Stahlman at the Massachusetts Institute of Technology, Cambridge 39, Massachusetts.

Addendum to Ons-en-Bray

The anemograph of Louis Léon Pajot, Count of Ons-en-Bray, was handsomely noticed in this Journal, 1957, *12*, 424-48 by H. E. Hoff, L. A. Geddes, and Roger Guillemin, to whom no account of the instrument apart from its original description was known. We are reminded, however, by Bruno Kisch that in the *Transactions of the American Journal of Cardiology*, 1952, *2*, 161 he gave, if not an account of the instrument, at least full credit to its inventor by stating that "graphic registration of periodic changes started in the 18th century in meteorology by a suggestion of the Marquis d'Ons-en-Bray, published in 1734 in the Mémoires de l'Academie."

The original description by Ons-en-Bray appeared in the *Histoire de l'Académie Royale des Sciences . . . Année M.DCCXXXIV, avec les Mémoires de mathématique et de physique pour la même année*, on pages 123-34 (and folding plates 5-10) of the Mémoires. This volume was published by L'Imprimerie Royale at Paris in 1736. Hoff, Geddes, and Guillemin used the Amsterdam edition of 1738. There was also a German translation of the Mémoires by Wolf Balthasar Adolph von Steinwehr which was published at Breslau in thirteen parts, 1748-59, under the title *Der königl. Akademie der Wissenschaften in Paris physische Abhandlungen, 1692-1741*. No copy of this translation is located for this country by the National Union Catalog. In it the article by Ons-en-Bray appears, according to information provided in the Supplement to Jöcher's *Allgemeines*

Gelehrten-Lexicon, 5 (Bremen, 1816), col.1118 and courteously verified by the British Museum, in Part 10 (1754), pp. 651-64.

Historical Gleanings

The activities of J. Lawrence Angel of Jefferson Medical College (*J. Hist. Med.*, 1957, *12*, 397-8) in studying skeletal remains at archaeological sites continue. In the summer of 1957 he spent some days with the expedition of Prof. Carl W. Blegen at the excavations of the Palace of Nestor at Pylos (*American Journal of Archaeology*, 1958, *62*, 175). In a paper presented at the Fifty-Ninth General Meeting of the Archaeological Institute of America and abstracted on page 221 of the same journal he reports on skeletons from the excavations of Prof. J. L. Caskey at Lerna. These showed marked traces of irregular nutrition, spinal arthritis, disk collapse, and high infant mortality. In all, Dr. Angel studied during the summer of 1957 almost four hundred Greek skeletons; his total is now more than two thousand.

The death of Giovanni Cardinal Mercati on 22 August 1957, at the age of ninety, removes from the world of scholars and librarians a renowned and beloved figure whose contributions to the literature have been immense. His bibliography in the latest edition by Augusto Campana, *Bibliografia degli scritti del Cardinale Giovanni Mercati* (Città del Vaticano, Biblioteca Apostolica Vaticana, 1957) includes 421 items for the years 1890 to 1956. Of these the following may be cited here as important to the history of medicine: "Pretesi scritti di Paolo l'Egineta e di Galeno.—Pretesi scritti di medicina greca" (*Bessarione*, 1916, *32*, 208-11 and also in his *Opere minori*, Città del Vaticano, Biblioteca Apostolica Vaticana, 1937, *3*, 521-4); *Notizie varie di antica letteratura medica e di bibliografia.* I. Filippo Xeros Reggino, Giovanni Alessandrino iatrosofista e altri nel codice Vaticano degli "Ephodia." II. Un nuovo capitolo di Sorano e tre nuove ricette superstiziose in Aezio. III. Su Francesco Calvo da Menaggio primo stampatore e Marco Fabio Calvo da Ravenna primo traduttore latino del corpo Ippocratico (Roma, Tipografia Poliglotta Vaticana, 1917 = *Studi e Testi*, 31), pp. 75 (1); "Altre correzioni ed aggiunte all'op. *Die Handschriften der antiken Ärtze*[!].—Per Aezio" (*Bessarione*, 1917, *33*, 50-4 and *Opere minori*, *4*, 16-21); "Altre correzioni ed aggiunte all'opera *Die Handschriften der antiken Aerzte* (*Bessarione*, 1917, *33*, 334-5 and *Opere minori*, *4*, 38-9); "Altre notizie di M. Fabio Calvo" (*Bessarione*, 1919, *35*, 159-63 and *Opere minori*, *4*, 65-9; "Francesco Calvo e Fausto Sabeo alla cerca di codici nell'Europa settentrionale" (*Atti della Pontificia Accademia Romana di Archeologia*, Ser. III, *Rendiconti*, 1937, *13*, 149-78, 248).

The exhibit of Central European manuscripts which was opened at the Pierpont Morgan Library in December, 1957, included a medical manuscript from the middle of the twelfth century which was purchased in 1927 by Mrs.

J. P. Morgan and was given to the library in 1952 by Mr. Junius S. Morgan and Mr. Henry S. Morgan. It is No. 16 in Meta Harrsen's *Central European manuscripts in the Pierpont Morgan Library* (New York, 1958). In its forty-three folios it presents three texts, viz., the *Epistula de phlebotomia* sometimes attributed to Roger of Salerno, which occurs also in the famous Laurentianus 73.1 of the late ninth or early tenth century; a charm in old German, *Ad restringendum sanguinem* (Longinussegen); and Macer Floridus, *De viribus herbarum*. In the eighteenth century the manuscript was in the possession of the Benedictine monastery of Abdinghof at Paderborn.

Lynn Thorndike, discussing in *Speculum*, 1957, *32*, 780-91 the thirteenth century manuscript at Berne (293, ff.1-75) which contains the anonymous *Liber de similitudinibus et exemplis*, includes (pp. 788-9) mention of certain passages in it which touch on physiology, hygiene, and medicine.

MS. D II 11 in the library of the University of Basel contains a number of medical and surgical tracts, portions of which have already been edited by Sudhoff and other historians. Folios 154-63 of this manuscript, which were written in old Provençal in the fourteenth century and which contain 121 medical formulas for the most frequent ills of man, have now been published by C. Brunel in *Romania*, 1957, *78*, 289-327. A glossary of terms is provided at the end.

The celebrations in honor of Poliziano in 1954 and in particular their medico-historical aspects have already been mentioned in these pages (1955, *10*, 120 and 256-7 and 1957, *12*, 87). It is gratifying to record now that the Atti del IV Convegno Internazionale di Studi sul Rinascimento were published in Florence (Sansoni) in 1957 under the title *Il Poliziano e il suo tempo*. There is much of interest to medical historians in this volume. In particular, the paper by Augusto Campana, "Contributi alla biblioteca del Poliziano" (pp. 173-229), is of value for its discussion of (i) Vaticanus latinus 6337 (pp. 185-98) and its relations with Laurentianus 73.1 and 73.41; and (ii) Poliziano's Apicius (pp. 198-217), first as it is represented in a fragment at Leningrad and in humanistic copies in the same Vatican manuscript, at Leningrad, and at Munich, and also as it is related to the Apicius at the New York Academy of Medicine.

"Gli statuti del Collegio dei Fisici di Cremona" are edited by Luigi Belloni, from the Latin MS. AG XI 60 in the Biblioteca Nazionale (Braidense) of Milano, in *Bollettino storico cremonese*, 1957, *20*, 5-46. These statutes were granted by Ludovico il Moro, Duke of Milano, on 5 September 1495. The edition is dedicated to Antonio Cazzaniga, native of Cremona, historian of medicine, and founder of the chair of legal medicine in the University of Milano.

Readers combing the excellent listing given by F. J. Norton in *Italian printers 1501-1520* (Cambridge Bibliographical Society, Monograph No. 3, London, Bowes & Bowes, 1958) will find included about fifteen medical

items which were published during these first two decades of the sixteenth century.

From 2 April to 31 August 1958 the Library of Congress has offered to visitors an outstanding exhibition entitled Early Printed Books of the Low Countries and consisting of 180 volumes drawn from its Lessing J. Rosenwald Collection and from a group of items purchased more recently by Mr. Rosenwald. This second group formed a portion of the library assembled by the Dukes of Arenberg. Several items of medical interest, including early editions of the *Regimen sanitatis Salernitanum, Dat regiment der ghesontheyt, Den groten herbarius,* and Ketham's *Fasciculus medicinae* were displayed, together with copies, thought to be unique, of Paracelsus, *Een constelijck distileer boec* (Antwerp, Jan Roelants, 1552) and the same author's *Een excellent traktaet leerende hoemen alle ghebreken der Pocken sal moghen ghenesen* (Antwerp, Jan Roelants, 1553). An attractive illustrated catalogue of the exhibition was prepared by Frederick R. Goff and is for sale by the Superintendent of Documents, Washington 25, D. C., at a price of 30 cents.

In *Bibliothèque d'humanisme et Renaissance,* 1958, *20,* 36-43 L. Elaut discusses three translations from Galen which Desiderius Erasmus did for his physician and friend Johannes Antoninus (1499-1543), native of Kaschau, who while studying at Basel had treated him for gravel and asthma. The three tracts, among the least medical of Galen's works and of special appeal to humanists, are *Paraphrasis in Menodoti exhortationem ad liberalium artium studia, De optimo docendi genere,* and *Quod optimus medicus idem sit et philosophus.* They were published by Froben at Basel in 1526 in an edition which is listed by F. Van der Haeghen in his *Bibliotheca Erasmiana* (Gand, 1893), IIe série, p. 26 and which is now uncommon. There is a copy of this edition at the Library of the College of Physicians of Philadelphia.

In 1527 and 1528 a plague then called *carbonchio* raged through Bologna and its environs and took in the city itself a toll of 13,000 victims. As an ex-voto for deliverance the citizens erected the church of S. Maria della Visitazione. This church, rebuilt in 1764, was recently declared inviolable when plans were drawn for a modern thoroughfare in its vicinity, and it will remain a monument to a tragic episode in medical history. The circumstances are related by Giuseppe Rivani in *La famèia bulgnèisa, Strenna 1958* (Bologna, 1957), pp. 152-9.

The American Historical Review, 1958, *63,* 654 and 675, carries reviews of two miscellanies which include articles on the history of medicine. In *Aspects de la propagande religieuse* (Studies by G. Berthoud *et al., Travaux d'humanisme et Renaissance,* vol. 28, Geneva, E. Droz, 1957) Elizabeth Feist Hirsch presents an account of the medical humanist Justus Velsius. In *Studia i Materialy z Dziejów Nauki Polskiej,* vol. 4 (Polish Academy of

Sciences, Committee on the History of Learning, Warsaw, State Publishing House for Scholarly Works, 1956), R. S. Bugay discusses the uses made of hot and mineral springs in Poland by physicians (e.g., Oczko, Syxt, and Petrycy) from the sixteenth to the eighteenth centuries.

Bern Dibner in *Agricola on metals* (Norwalk, Conn., Burndy Library, 1958) presents in splendid clarity and beautiful format the substance of Georgius Agricola's great *De re metallica,* which was first published in March, 1556 (Basel, Froben), between three and four months after the death of its author. The tribute is fittingly dedicated "To Herbert Clark Hoover, engineer, statesman, devoted citizen, whose translation [London, The Mining Magazine, 1912] of Agricola's work has linked engineers of four centuries ago with those who continue their quest of knowledge of our Earth in this, the third International Geophysical Year." Medical historians, remembering that Agricola was primarily a physician, will find of special interest the remarks on ventilation and on miners' diseases.

Luigi Belloni's article "Notizie storiche sulle fistole biliari" (*Archivio ed Atti della Società Italiana di Chirurgia,* 58° Congresso, Milano, 1956, *1,* 477-86) reproduces with other significant illustrations a "Calculus excisus à Valopio inter alios 123." The source on this stone which was found by Falloppio is the physician Johann Kentmann of Dresden, who provided the illustration in his *Calculorum qui in corpore ac membris hominum innascuntur, genera XII* (Zürich, 1565), f.6b. He states on f.9a that Falloppio removed the stone at the last dissection which he performed at the University of Padua, a short time before his death in 1562. There is further mention of Falloppio on f.5a.

The Times Literary Supplement for 23 May 1958 carries a feature article, "Flamboyant Bibliophile," which reviews Vittorio Gabrieli's *Sir Kenelm Digby: un inglese italianato nell'età della controriforma* (Roma, Edizioni di storia e letteratura).

The year 1958 marks the third centenary of the birth of Luigi Ferdinando Marsigli, who throughout his military career nurtured the love for natural history which he had first displayed in his student days at Padova and Bologna. His career is summarized by Piero Biffi in the above-mentioned issue of *La famèia bulgnèisa,* pp. 28-39.

The Clendening Medical Library of the University of Kansas School of Medicine has published an interesting catalogue (1958) of its exhibit on Athanasius Kircher (1602-80). The twenty-nine books listed were gifts from the collections of Ralph H. Major, Russell L. Haden, and Logan Clendening. The catalogue reproduces a letter by Kircher which was recently acquired; written from Rome on 13 February 1667 to Ovidio Montalbani in Bologna, it touches on a subject dear to Kircher's heart, Egyptian obelisks, and mentions his "CHINA ILLVSTRATA" and *Ars magna sciendi.*

Marjorie Hope Nicolson in *Conway letters* (New Haven, Yale University Press, 1930) gave repeated attention to the headaches of Lady Anne, Viscountess of Conway, and the many attempts of celebrated physicians, including William Harvey and Francis Mercury van Helmont, to alleviate them. They are further examined by Grace B. Sherrer in an article "Philalgia in Warwickshire" (*Studies in the Renaissance*, 1958, *5*, 196-206). The article bears the subtitle "F. M. Van Helmont's anatomy of pain applied to Lady Anne Conway" and prints in Latin text and English translation a poem of 142 lines, dedicated to Lady Anne, which is preserved at the Bodleian Library, MS. Locke c. 32, fol. 47. This poem gives a fuller statement and application of Van Helmont's theory of the nature of pain than does the British Museum MS. Sloane 530. The author of the article would attribute it to Van Helmont himself, rejecting Henry More for the reasons that "More's principal poetic work was accomplished in the years before his friendship with Anne Conway" and that the theme is absent from his writing after he knew Van Helmont, the only work in which he considers a medical theme being his " 'Circulatio Sanguinis,' 106 lines in Latin inscribed 'Ad celeberrimum Medicum G. Harvaeum Inventorem' and printed in *Carmina quaedam*, published in the year of Lady Conway's death."

On 29 April 1958 Le Théâtre du Nouveau Monde, a Montreal troupe, opened a run at the Phoenix Theatre in New York with the presentation of Molière's *Le malade imaginaire*. This play has already figured in these pages (1953, *8*, 88 and 220) with reference to clyster buttons.

Thomas Dover and his activities with buccaneers have come into considerable prominence of late. In this connection historians (and in fact all physicians who must wrestle with the complexities of workmen's compensation) are reminded of Saul Jarcho's article, "Workmen's compensation among pirates and buccaneers," in the *Academy Bookman*, 1956, *9*, 5-8.

It is known from duplicate records which John Locke kept of his library that at its fullest extent in the 1690's it numbered some 3,210 titles and 3,390 volumes. Of these, some 820 are now in the possession of the Earl of Lovelace. Almost ten per cent of the titles were medical. Collectors who may wish to know how to recognize the many volumes which were scattered should read the article by J. R. Harrison and Peter Laslett, "The library of John Locke," in *The Times Literary Supplement* for 27 December 1957, and should digest well the peculiar system which Locke followed in marking his books. To every volume he gave a press-mark, which was entered inside the front cover near his signature and was also pasted on the spine. It consisted of a number between 3 and 22 for shelf or box and a running-number beneath for the item on the shelf or within the box. But a volume of Locke's can be identified even if the covers are gone, for the last two digits of the date on the title were carefully underlined (the last three if the date was earlier than 1600), and the final page number was overlined. Moreover, if

Locke had purchased the book, he entered in the bottom margin of page 11, to the left, the digit for pounds, in the middle that for shillings, and to the right that for pence. Messrs. Harrison and Laslett at Trinity College, Cambridge, will "be pleased to hear of *any* Locke holding."

An unsigned note in *Notes and Queries,* New Series, 1958, 5, 26-35 discusses the occurrence of the putrid fever at Robert Peel's Radcliffe mill in 1782-4 and concludes that the outbreak has been greatly exaggerated. At the time, it was used to criticize the way in which Peel managed the mill, and there was an investigation, with recommendations and correspondence in the columns of Manchester newspapers, in which Dr. Thomas Percival and other physicians had a part. Since then biographers and historians of factory legislation have recounted the episode to the detriment of Peel's character. As a matter of fact, the putrid fever was usual wherever the poor were gathered in numbers, and according to the note the outbreak at Radcliffe was in no way remarkable, so that it is unjust to blame Peel.

In Ireland in the year 1845 the potato was the sole food of about one-third of the population and the main article of diet of a much larger number. The fungus disease which attacked it in that year brought on quickly the Great Famine which is examined in *The great famine: studies in Irish history, 1845-52,* edited by R. Dudley Edwards and T. Desmond Williams (New York, New York University Press, 1957). These studies, a co-operative effort of scholars in Ireland and Great Britain, were originally published in Dublin. In Chapter V, pp. 263-315, the "Medical history of the famine" is carefully and capably discussed by Sir William P. MacArthur, M.D., D.Sc., F.R.C.P., Clinical Lecturer in Tropical Medicine at the University of Oxford and formerly Director-General of the British Army Medical Services. He describes the diseases—typhus, relapsing fever, dysentery, and scurvy—which became widely epidemic in the famine, investigates the conditions in hospitals, workhouses, and gaols, and surveys the medical organization and arrangements, the fever acts, and the efforts of the Board of Health and local authorities to limit the spread of infection. A separate section is devoted to the outbreak of cholera which, starting in December, 1848, reached its height in May, 1849 and began to decline in June. The Great Famine is of interest in this country not only for its medical and humanitarian aspects but also for the impetus which it gave to Irish emigration to the United States.

An epidemic of cholera which struck the city of Bologna in December of 1854 and terminated only in November of 1855 is recalled by P. Donato da San Giovanni in Persiceto in his article "I Cappuccini e il colera a Bologna nel 1855," in *Strenna storica bolognese,* Anno settimo (Bologna, Tipografia Luigi Parma, 1957), pp. 259-76. Deaths during the epidemic totaled 3,649. Eighteen Capuchins ministered spiritually to the sufferers. The article describes their services and furnishes illustrations and documentation on the epidemic.

The centennial of the publication of Rudolf Virchow's *Cellular pathology* is being celebrated throughout the year 1958 by the Armed Forces Institute of Pathology in Washington. Lectures to be delivered from time to time will stress the history and theory of cellular pathology and the applications of current research to it, as in histochemistry, biophysics, light and electron microscopy, virology, nutrition, immunology, and ionizing radiation.

A second centenary of interest to historians of medicine occurs in 1958. To commemorate the pioneering papers of Friedrich August Kekulé and Archibald Scott Couper on the tetravalence and self-linking of carbon, a centennial celebration will be held at the fall meeting of the American Chemical Society in Chicago 7-12 September. Details of the celebration may be obtained from O. T. Benfey, Department of Chemistry, Earlham College, Richmond, Indiana. The event will be commemorated also in London and in Ghent.

In *La revue des deux mondes* for 1 February 1958, pp. 477-92, Guy Godlewski contributes under the title "Claude Bernard intime" to our knowledge of the biography of the famous scientist, examining in particular his troubled domestic relations.

The distaff side received recognition at the College of Physicians of Philadelphia, Section on Medical History, when on 21 January 1958 the entire program was presented by the Woman's Medical College of Pennsylvania and on 15 April the Kate Hurd Mead Lecture of the same institution was delivered by Eleanor M. Tilton, biographer and bibliographer of Oliver Wendell Holmes and author of *The Amiable Autocrat.*

At the 51st Annual Meeting of the Southern Medical Association at Miami Beach, 12 November 1957, Dr. Pedro Nogueira of Cuba presented the facts on the restoration of Camp Lazear to the present time and outlined plans for future preservation of the area as an historic site. It is hoped that Cubans and Americans will co-operate in its maintenance.

This department is indebted to David L. Cowen for several references to articles on the history of medicine which have appeared in nonmedical journals. In *American Antiquity,* 1958, *23,* 297-308, J. Eric S. Thompson examines, under the title "Symbols, glyphs, and divinatory almanacs for diseases in the Maya Dresden and Madrid codices," the scenes and parallel passages in these manuscripts which represent a goddess or woman in association with various birds, animals, and gods. Edmund S. Morgan suggests in *The William and Mary Quarterly,* Third Series, 1957, *14,* 414-7, that the artist John White, who appeared prominently on the historical scene as a member of the expeditions sent by Sir Walter Raleigh to Virginia and whose great water colors of the Indians are well known, was a means of contact between the herbalist John Gerard and the Roanoke colonists. It had already been noted that his drawing of the milkweed appears in Gerard's

Herball (1597). In addition, Gerard incorporated in his chapter on bindweed or smilax, where sarsaparilla is designated as *smilax Peruviana,* a short description which had been reported to him by White. It is obvious that White mistook some other plant for sarsaparilla, and of sarsaparilla itself the passage tells us nothing, but it does prove that he had been watching for the plant in Virginia, possibly on the request of Gerard. The difficulty of evaluating properly the importance in urban sanitary reform of Edwin Chadwick and his colleagues on the one hand and Thomas Percival and the early pioneers on the other hand is again clear from E. P. Hennock's "Urban sanitary reform a generation before Chadwick?" (*Economic History Review,* Second Series, 1957, *10,* 113-9. The article answers B. Keith-Lucas's observations in the same journal (1954, *6,* 290-6) and pleads for retention of the current emphasis on the 1840's in this matter. On p. 120 a note by Mr. Keith-Lucas is appended. It is pleasant to record here the appearance in *Cahiers d'histoire mondiale,* 1957, *3,* 881-908, of Richard H. Shryock's "Nineteenth century medicine: scientific aspects."

A sale at Marburg on 9 November 1957 from the estate of Duke Charles Edward of Saxe-Coburg included a letter from Voltaire to his physician Théodore Tronchin, a half-page document signed by William Harvey, and a letter from Edward Jenner to his son (*Times Literary Supplement,* 7 March 1958). On 19 November a letter of Lavoisier, Paris, 8 October 1791, was sold in Paris; it was written to a relative and concerns money matters (*ibid.*). Manuscript papers of the Herschel family and books from the Herschel library realized £14,450 at a sale at Sotheby's on 3-4 March 1958; these included a presentation copy of the first edition of *The origin of species* "containing a letter from Darwin to Sir John Herschel speaking of the deep impression made on him in early life by Herschel's *Introduction to natural philosophy*" (*TLS,* 2 May). Several monuments of medicine were included in the first portion of the library of Mrs. Hilda Leyel, which realized £14,268 at Sotheby's on 14-16 April (*TLS,* 23 May).

He who sails the Ocean Sea need not be removed from the history of medicine. In *one* Atlantic crossing recently the flagship of a foreign line, left nameless here to avoid closing this note with the admonition [ADVT.], offered in its daily news sheet items on the fees allotted surgeons in the Code of Hammurabi, healers of ancient Egypt, the treasure of Paracelsus, Max Samson's drug store in New Orleans, and the Armed Forces Medical Museum.

Dr. Frank B. Rogers and the editor of this department have written a short history of the National Library of Medicine which is published in two parts in the *Library Quarterly,* 1958, *28,* 1-17 (January) and 95-121 (April).

Shortly before his death Arturo Castiglioni, of affectionate memory, furnished to the Journal (1953, *8,* 217-9) an account of the new Museo Nazionale della Scienza e della Tecnica in Milano. The museum, still under

the devoted guidance of Guido Ucelli, celebrated its fifth anniversary on 2 March 1958. The President of the Italian Republic attended the ceremonies. In five years the number of visitors has risen from 60,000 annually to 330,000, and the museum has become increasingly, in honor to Leonardo, a national and international center for the history of science and technology.

Dr. Dirk Arnold Wittop-Koning, who operates a pharmacy in Amsterdam, teaches history of pharmacy at the Municipal University of Amsterdam, and is curator of the Dutch Medical-Pharmaceutical Historical Museum, has been named the recipient of the fourth George Urdang Medal by the American Institute of the History of Pharmacy. This medal, established in 1952 on the seventieth birthday of Dr. George Urdang, has not been awarded since 1955. It is given "for original and scholarly publications or series of publications pertaining primarily to historical or historico-social aspects of pharmacy, appearing anywhere in the world."

Seasonable for mention in a July issue, and perhaps of very practical preventive value to vacationing historians of medicine, is the article by Saul Jarcho and Anastasia van Burkalow, "A geographical study of 'swimmers' itch' in the United States and Canada," which appeared in the *Geographical Review,* 1952, *42,* 212-26. This cercarial or schistosome dermatitis is a disease of place, and the major endemic area includes Michigan, Wisconsin, Minnesota, and sections northwestward into Canada.

A new volume (Supplement II) in the series *Dictionary of American biography* was published in May by Charles Scribner's Sons. Sponsored by the American Council of Learned Societies and edited by Dr. Robert L. Schuyler and Dr. Edward T. James, it presents 585 biographical articles on individuals who died during the five-year period 1936-40. Biographies of anatomists, bacteriologists, dermatologists, neurologists, ophthalmologists, pathologists, pediatricians, physiologists, psychiatrists, surgeons, etc. are included. Authors of the articles were chosen on the basis of their particular knowledge of the subject's work and personality; one notes among historians Chauncey D. Leake for William Snow Miller, George W. Corner for Charles R. Stockard, George Rosen for William Hallock Park, Walton B. McDaniel 2d for George Edmund de Schweinitz and Henry R. M. Landis, John F. Fulton for Harvey Cushing and J. G. Dusser de Barenne, Winfred Overholzer for William Alanson White, Otto Glasser for Francis Henry Williams, Helen Clapesattle for Charles H. and William J. Mayo, and Esmond R. Long for Lawrason Brown.

The *Bulletin of the History of Dentistry,* now in its sixth volume, continues to issue monthly under the auspices of the American Academy of the History of Dentistry and the efficient editorship of Dr. George B. Denton, American Dental Association, 222 Superior St., Chicago 11, Illinois. Published usually on two well mimeographed and easily legible pages, it combs

current literature in the history of dentistry in this country and abroad and presents significant items of news interest.

A new journal of the history of medicine makes its appearance with the first issue of *Revista de la Sociedad Cubana de Historia de la Medicina,* Enero-Marzo, Habana-Cuba, 1958. The society for which it is the official organ was founded two years ago. The director of the journal is Dr. Horacio Abascal, its administrator Dr. Esteban Valdés-Castillo y Moreira, and its chief editor Dr. César A. Mena Serra; the editorial board includes also Dr. Saturnino Picaza and Sr. César Rodríguez Expósito. The journal is a quarterly and the price of subscription is $2.00 per year. Correspondence should be addressed to Edificio de la Academia de Ciencias, Cuba No. 460, Habana-Cuba.

First Annual Victor Robinson Lecture, Temple University

George Rosen, M.D., Ph.D., will give the First Annual Victor Robinson Lecture in the History of Medicine at Temple University School of Medicine on 23 September 1958. His topic will be "Victor Robinson—A Romantic Medical Historian." The lecture will be presented in the medical school auditorium from 4-5 p.m.

This lectureship, named in honor of Dr. Victor Robinson, Professor of the History of Medicine at Temple University from 1929 until his death in 1947, is appropriately initiated by Dr. Rosen, a personal friend of his. Dr. Rosen serves on the medical faculty of Columbia University and is editor of the *American Journal of Public Health.*

The William Osler Medal of the American Association for the History of Medicine

All medical students of the United States or Canada are invited to submit essays in competition for the WILLIAM OSLER MEDAL of the American Association for the History of Medicine.

This medal is granted annually to the author of the best essay submitted during the year. The purpose of this prize is to stimulate interest and research in medical history among medical students. The medal has been named in honor of Dr. William Osler, the Great Physician who generated a love for the humanities among students and doctors alike.

The Association will consider unpublished essays by men and women who, at the time of writing the essays, are students in schools of medicine in the United States or Canada. Essays must be submitted before graduation, or within one year after graduation.

Essays must be the result of original research or show an unusual appreciation and understanding of historical problems. Since the rules stipulate that the essays must be original, contestants may not receive help

from sources other than the usual courtesies extended by faculty advisers, libraries, museums and the like, which are open to all investigators.

Essays should be submitted before April 1, 1959 to:

Samuel X Radbill, M.D.
7043 Elmwood Avenue,
Philadelphia 42, Pa.

The Schuman Medical Historical Essay Prize

Mr. and Mrs. Henry Schuman of New York offer annually a prize of $250.00 for an original essay written in English, *not to exceed 10,000 words,* on some subject pertaining to the history or philosophy of medicine.

To be eligible, an essay must be written by a doctor of medicine or of one of the medical sciences who has taken his or her M.B., M.D., or Ph.D. degree not more than five years prior to submission of the paper. The competition is open to candidates of any country. Essays which have won awards in other contests are not eligible. The prize-winning essay will be published in the *Journal of the History of Medicine.* The judges reserve the right to withhold the prize in any year if no essay is deemed of sufficient merit.

Entries should be submitted on or before 1 May 1959 to Dr. John F. Fulton, Yale University School of Medicine, 333 Cedar Street, New Haven 11, Connecticut, U.S.A.

Book Reviews

E. Newton Harvey. *A History of Luminescence from the Earliest Times until 1900.* Philadelphia, The American Philosophical Society, 1957. 692 pp., illus. $6.00.

Reviewed by R. L. Airth, *The McCollum-Pratt Institute, The Johns Hopkins University, Baltimore, Maryland.*

It is fitting that The American Philosophical Society should, in part, defray the expenses of publishing this book for one of its founders, Benjamin Franklin, made original observations in the field of luminescence. It is also fitting that Dr. E. Newton Harvey should write a history of luminescence, for he has written extensively about and conducted many experiments in this field.

According to Harvey "the purpose of this book is to trace the discovery and ideas regarding these weak lights without heat (luminescence) from the earliest times until the end of the nineteenth century." This objective has been most adequately and extensively fulfilled. The effect of luminescence on the thinking and actions of man has been and is immense. Harvey notes the mythological beliefs of the Altaic race regarding the occupation of the earth by people who gave out light before the appearance of the sun and moon; the impact of light emission by the firefly on Chinese and Japanese writings; the Biblical references to light emission from the human body; the many references in classical literature to luminescent phenomena. The impact of present-day luminescent processes, however, such as television, is left to future historians.

On reading this book one is struck by the truth of Harvey's statement "the history of luminescence is a guide to the history of science in general." The development of our present-day knowledge of luminescence has depended upon and has stimulated modern concepts of light, electricity, and photochemistry. Indeed, one can gain some insight into the breadth of this book by noting that the pertinent experiments and writings of such men as Bacon, Becquerel, Cavendish, Crookes, Faraday, Galvani, Hooke, Malpighi, Newton, and Priestley, to mention a few, are considered.

A History of Luminescence is divided into three parts. The first part considers in chronological fashion the development of the entire subject of luminescence. Starting with the eastern observations and writings of antiquity, Harvey considers the development of his subject through the middle ages, fifteenth, sixteenth, seventeenth, and eighteenth centuries and up until the end of the nineteenth century. The latter times deal with the Western European investigations.

Part II considers the luminescence of nonliving material. In this section the growth of ideas concerning electroluminescence, phosphorescence,

fluorescence, thermoluminescence, triboluminescence, crystalloluminescence, lyoluminescence, radioluminescence, and chemiluminescence are considered. Once again each subject is treated historically and many of the observations mentioned in the first part are considered in greater detail in this section. Harvey has struck such a balance between quotations of original work and his own comments that the uninitiated should not lose the train of thought and the initiated should not lose interest in the book.

The last part of the book considers the luminescence of living systems or bioluminescence. The author first considers the plant bioluminescent forms, namely, bacterial and fungal light emission. The phosphorescence of the sea is then considered. Other than the Aurora borealis probably no light phenomena has been considered more broadly and extensively. The last part of this book deals with the luminous terrestrial and fresh water forms.

This book should be of interest not to the students of luminescence alone but to any one interested in the evolution of scientific concepts. The approach is very similar to that of the Harvard case histories and as such could make a good working text for science students. The main objection of the reviewer is to the brevity of the subject index. The indexing is broad enough, however, so that minor points can be found with a little searching.

JOSEPH NEEDHAM. *Science and Civilisation in China.* Vol. 2, *History of Scientific Thought.* Wang Ling, research assistant. New York, Cambridge, University Press, 1956. 696 pp. Illus. $14.50.

Reviewed by FRANZ MICHAEL, *Far Eastern and Russian Institute, University of Washington, Seattle.*

IN the second volume of his contemplated seven-volume work on *Science and Civilisation in China,* Dr. Needham undertakes to describe the philosophical and general intellectual setting of Chinese scientific thought. He gives his interpretation of the main philosophical schools, the Confucian, the Taoist, the Mohist, the Logician, and the Legalist, and against this background discusses the fundamental ideas of Chinese science selected from a vast area of source material. Continuing a description of the main trends of Chinese philosophical thought, Needham takes up the influence of Buddhism, of neo-Confucian philosophy, and of the Chinese concepts of the laws of nature on the development of thought in China in a detailed discussion which, at every turn, attempts to compare and contrast the Chinese story with that of the West. This is a work rich in material, containing many interesting ideas and, often, aesthetic insight into different aspects of Chinese culture which influenced and impressed the author during his studies in China and his years of contact with Chinese scholars.

What mars the value of this work is the author's conceptual framework. Upon all intellectual history in general and upon the Chinese in particular,

Dr. Needham imposes his Marxist doctrinal view. In order to make Chinese history fit his preconceived notions he introduces the terminology used by Communist historians for the stages into which they divide Chinese, as well as all other, history, without ever bothering to document, let alone prove, the existence of these stages of social history which are simply given as facts. Indeed, they are in some cases so unrelated to the material introduced by the author himself that it is hard to see how the author can have failed to see some of the resulting contradictions. What is worse, in trying to prove his points about the content of the philosophical statements, the author uses interpretations and translations of Chinese texts by men whose views are biased to say the least and who, like Needham himself, do not hesitate to read into the texts ideas which to any nonbeliever simply are not there.

Like all doctrinal histories, Needham's story becomes melodramatic. The villains are the Confucianists, who represent a "feudal society" or at times a "feudal bureaucraticism" (brought about with the help of the Legalists) and who are anti-scientific. The heroes are the Taoists, who "intuitively went to the roots of science and democracy alike." Their "female receptiveness . . . in their observation of Nature was inextricably connected with the feminine yieldingness which they believed should be prominent in human social relations." This brought them into "opposition to feudal society" and this "yieldingness" was "in a real sense the poetical expression of a co-operative collectivist society." (The footnote to this remarkable line of thought deals with the question of whether ancient Chinese society was matriarchal or not.) Needham states as if it were unquestionable his assumption that there had been "collectivism of the villages" in ancient time "and that it was to exist again (though the Taoists could not know that millennia must elapse before humanity would return to their ideals)." To Needham it is self-evident that socialism holds the future, and that science and socialism are identical. The Taoists had the understanding of both: "conducting a socialist holding action for two thousand years and condemned to perpetual heterodoxy, Taoism had to retain, unborn within itself, science in the fullest sense." It is then no surprise that Needham is capable of finding traces of "some close connection between them [the Taoists] and the working people." He also quotes Engels, Plekhanov, and Marx for the thought that by the study of nature and by submitting to her man increases his power over nature and thus enlarges his freedom—a thought which, to him, has a Taoist ring.

These are but a few examples of Needham's approach. His book is an interesting partisan effort, containing a great amount of material never brought together before; but in spite of its pretenses its scholarly value is negated by its doctrinal bias, and its shortcomings indicate the faulty conceptual structure of the volumes yet to come.

Die Illustrationen des Arzneibuches der Periode Shao-hsing (Shaohsing pen-tsáo hua-tú) *vom Jahre 1159.* Ausgewählt und eingeleitet von Otto Karow. [Leverkusen, Farbenfabriken Bayer A.G.] 1956. 83 pp., illus.

Reviewed by ILZA VEITH, *Associate Professor in the History of Medicine, University of Chicago, Chicago, Illinois.*

THE Pharmazeutisch-Wissenschaftliche Abteilung (pharmaceutical scientific department) of the Bayer factories in Leverkusen have enriched medico-historical literature with many extremely valuable publications. In 1955 they sponsored the publication of a volume entitled, *Ärzte auf Forschungsreisen,* which contains important sections of the travel diaries of European physicians to Japan, Africa, Siberia, and the New World, ably edited by Dr. G. H. Klöverkorn. Their *Therapeutische Berichte* regularly contain beautifully illustrated articles on medical history; and the leaves of their medico-historical calendar of 1956 are timeless and valuable additions to the picture collection of medical historians.

In their most recent publication the Bayer Werke again demonstrate their great interest in the cultural and artistic aspects of the history of medicine; and the resulting volume is as beautifully produced as it is informative. Its subject is the most famous work on Chinese pharmacology, the *Pên Tsáo,* or "Great Herbal." Tradition attributes its authorship to Shên-nung, the "Divine Husbandman," one of the three legendary God-emperors of the third millenium B.C. Makers and sellers of Chinese drugs even today venerate him as their divine patron; and peasants and farmers see in him the father of agriculture. Like Mithridates, Shên-nung is reputed to have tested on his own body all substances of the animal, mineral, and vegetable kingdoms, and the experiences gained thereby are believed to have formed the basis of his canon, the "Great Herbal."

There is general agreement that Shên-nung's existence and his authorship of the *Pên Tsáo* belong to the realm of legend, and we are indebted to Dr. Otto Karow of the University of Bonn for a brief but excellent reconstruction of the actual history and contents of this great work. For many years Dr. Karow has applied his extensive knowledge of Japan's language and history to the study of Japanese medical literature and has thereby added greatly to our understanding of Far Eastern medicine in general, since the early medicine of Japan is largely derivative of the Chinese.

The fragments of the *Pên Tsáo* which are the basis of the book here reviewed have survived the ages as parts of Japanese manuscripts and probably stem from the twelfth century edition of Wang Chi-hsien. The outstanding feature of his edition is its magnificent illustrations, 30 of which are reproduced in Dr. Karow's work and are thus for the first time available in a Western publication. These woodcuts are of exceptional artistic beauty and their subject matter is extremely varied since medicinal substances were derived from a great number of sources. Dr. Karow's translation of the Chinese names of the plants and animals and his summaries of their

pharmacological applications add further value to this beautiful book. There is little doubt that all those who were fortunate enough to receive copies of Dr. Karow's edition will join this reviewer in the hope that the Bayer Farbenwerke will continue in its valuable program of medico-historical publications.

MICHAEL GRANT. *Roman History from Coins.* Cambridge, University Press, 1958. 96 pp., 32 plates. $2.75.

Reviewed by BRUNO KISCH, *Curator of the Edward Clark Streeter Collection of Weights and Measures, Yale University School of Medicine.*

IN the period from the sixteenth to the nineteenth century, numismatists and especially the collecting and studying of Greek and Roman coins was a favorite hobby of interested middle-class Europeans as well as of the nobility. The late king Victor Emanuel IV of Italy, for example, was well known as an excellent numismatic scholar. This hobby became less and less popular in the twentieth century, mainly because it calls for wide historical and numismatic knowledge and for serious studies.

The present book appears to be an attempt to make classical numismatics again palatable to the bourgois intelligencia. It is easily readable even for those with not the slightest pretence to classical knowledge. It amuses without making any demands. But still it is apparent from each page that the author is a great scholar in classical history as well as in numismatics.

In previous numismatic literature it has been repeatedly pointed out how Roman rulers have used their coinage as a means of political propaganda. Grant elaborates very interestingly on this point. Of course, this trend was not an invention of the Roman emperors. From the time of the republic, we know, brass ingots commemorated Roman victories over Samnium or over the Parthians, as Haeberlin has shown in his classical work on early Roman heavy brass money. But Grant, who confines his book mainly to Roman imperial coins, discusses his topic also from other viewpoints. We learn from the book how coins are sources of evidence for the existence of historical personalities whose presence and role in Roman history are otherwise not traceable. Not only the features of historically known personalities are presented on the coins, but also real true-to-life portraits from which one can even learn details about the hair style of Roman ladies in different centuries. The coins inform us also about the appearance of buildings and statues no longer in existence.

Every single chapter of the book is fascinating reading and the 32 plates which it contains present excellent photographs of all the coins discussed in the text.

Communicationes ex Bibliotheca Historiae Medicae Hungarica. Az Országos Orvostörténeti Könyvtár Közleményei. Budapest, 1955-1956, nos. 1, 2, 3.

Reviewed by GEORGE ROSEN, *Professor and Head of the Division of Public Health Education, Faculty of Medicine, Columbia University, New York City.*

THESE are the first three issues of a journal to be devoted to the history of Hungarian medicine and related subjects. The publication will appear from time to time. Practically all the articles are in Hungarian, except for two in German and one in English. At the end of each article, however, there are résumés in Russian or German, and in most instances, in both languages. The three issues contain the following papers:

Number 1:

Gyula Jáki: Examination of barber-surgeons in the eighteenth century.

Geza Hahn: Development of the materialist trend in Hungarian medicine.

József Bencze: 700 years in the history of the hospitals of the Vas district.

Aurel Vajkai: Historical development of Hungarian spas.

István Kerdö: Hippocrates and bioclimatology.

György Huszár: A bibliography of Hungarian odontology.

Number 2:

György Gortvay: International losses in medical history after 1945 (necrologies).

Miksa Natter-Nad: The use of juniper from antiquity to the present.

Alfred Berndorfer: Die medizinischen Briefe des ungarischen Humanisten Andreas Dudith (1533-1589).

Ákos Palla: The *puteus leprosorum.*

Ibolya Katona: The first Hungarian woman physician.

Andras Daday: The first historical account of Hungarian spas.

Lászlo Mosonyi: The dialectic element in the work of Sandor Koranyi.

Number 3:

Franz Kiss: Stephan van Apáthy als Neurolog.

J. Balogh: Hungarian traumatic surgery in the first part of the nineteenth century.

Andor Olah: Popular healing, medical history and medical science.

Peter Balazs: Periodic public health reports in our libraries from the end of the eighteenth century.

Gedeon Borsa: Gergely Frankovith (author of the first Hungarian medical book).

GEORGE HAINES, IV. *German Influence upon English Education and Science, 1860-1866.* New London, Conn., Connecticut College, 1957. xii, 60 pp., 2 charts.

Reviewed by WILLIAM B. WALKER, *Gstaad, Switzerland.*

IN this short but thoroughly documented study the author, Professor of American History at the University of Connecticut, gives further evidence of the value of the perspective of the social and cultural historian for matters of scientific interest. Citing examples of reform from several fields, he notes that ". . . the renaissance in physiological studies was due primarily to . . . William Sharpey . . . and William Carpenter . . . ," both of whom through study or reading had contact with contemporary German thought. Similarly, from his chair of Chemistry at Giessen, Justus von Liebig appears as preceptor to many of the succeeding generation of English chemists. In the field of biology, Professor Haines traces the development in dialectic of ". . . the German concept of evolutionary development . . . ," notably by Oken, juxtaposing rather than relating this to ". . . Darwin's discovery . . . of a . . . naturalism in the process of organic evolution. . . ."

Turning to developments in education, the contrast between the activity of independent groups such as the Warrington Academy and the Lunar Society of Birmingham, and the relative inertia of the Universities is used to emphasize the ". . . leadership by Scotsmen and the Scottish educated . . . ," who were aided by the international character of Calvinistic Protestantism to abet their contacts with continental centers of learning. Subsequent reform is exemplified by the work of Henry Brougham in founding London University, and more widely by Thomas Arnold of Rugby, who through acquaintance with ". . . the depth and research of German literature . . ." was inspired not only to extend the curricula of the public schools to include modern history and languages, but also to urge a ". . . national and systematic effort . . ." to provide the advantages of a higher education for the middle classes.

This theme was to dominate the writings of those who attempted to propound a scheme of education for England. An early protagonist, Edward Bulwer-Lytton, urged state patronage to afford scientists ". . . a wider and more ready sphere . . ." than private auspices could provide. Noting also that England had many scientists of ". . . the first order of mind . . . ," he argued that at present ". . . their light is not generally diffused . . . ," and advised development of ". . . a National Education . . ." to train also those minds who apply ". . . to the discovery of less general principles . . . ," and those in turn who apply ". . . principles already discovered to purposes of practical utility. . . ." Yet these and other pleas for institutionalizing the country's educational and scientific life provoked far slower response than individual reforms; the lag here exemplified between German example and English experience was not appreciably closed until the late 1860's.

Professor Haines concludes by suggesting that this phenomenon may have imposed a handicap on Britain's later attempts to maintain herself as a world power; moreover, this experience may have further value for the present day, when doubts exist as to the adequacy of American educational institutions in the face of the Soviet threat. The ability of this modest monograph to use the medium of history to focus our attention anew on a vital current problem should recommend it not only to historians of medicine and science but also to all thoughtful readers.

JOHANNES KARCHER. *Theodor Zwinger und seine Zeitgenossen. Episode aus dem Ringen der Basler Ärzte um die Grundlehren der Medizin im Zeitalter des Barocks* (Studien zur Geschichte der Wissenschaften in Basel herausgegeben zum fünfhundertjährigen Jubiläum der Universität Basel 1460-1960). Basel, Verlag von Helbing & Lichtenhahn, 1956. 75 pp.

Reviewed by GEORGE ROSEN, *Professor and Head of the Division of Public Health Education, Faculty of Medicine, Columbia University, New York City.*

IN the later sixteenth century the medical faculty of the University of Basel had as its three luminaries, Felix Platter (1536-1614), who taught the practice of medicine; Caspar Bauhin (1560-1624), as professor of anatomy and botany; and Theodor Zwinger (1533-88), who had the chair of medical theory. A study of Platter was issued by Dr. Karcher in 1949, and we now have from his pen a companion piece on Zwinger and his contemporaries. This is not a biography. While employing biographical data, attention is focused chiefly on medical theory and its problems during the second half of the sixteenth century.

This was a transitional period in medicine. Humanistic physicians had made available good texts of the ancient medical writings and had provided them with useful commentaries. Anatomy had been placed on the objective basis of dissection and direct observation by Vesalius. Numerous clinical observations were made and reported. Nonetheless, there was as yet no theoretical synthesis which was generally accepted. This is painfully evident when medical men were confronted by epidemics and the urgent tasks of community protection. To paraphrase Juvenal, the problem was who would guide the experts called upon for guidance; and a number of contending groups claimed to be able to do so. This was particularly so in relation to the Paracelsian doctrines. After tracing the backgrounds and ideas of Zwinger and his contemporary Johannes Crato von Krafftheim (1519-1585), Karcher develops the revival of Paracelsism in Basel and shows how Zwinger reacted to this situation. Finally, he describes the plague epidemic of 1563-64, and how Zwinger and Crato reacted to it. The former attributed it to a pestilential constitution of the air, the latter to the inhalation of plague seeds

exhaled by plague patients. Throughout, the author paints a picture of confusion and uncertainty in the face of a need for action. This monograph is a useful contribution to the history of medicine. It will be of particular interest to those concerned with the history of epidemiological theory, and of medicine in the sixteenth century.

There is one aspect of this study, however, which requires somewhat closer attention. In his subtitle, Karcher refers to his essay as an episode in the history of medicine in the age of the Baroque. Furthermore, in the preface, the medical developments described in the study are related to the spirit of the Baroque. This attribution occurs at various points in the volume (pp. 20, 30, 46, 64-65), indeed, even Paracelsus is connected with the Baroque! At this point, the reader may well raise an eyebrow and ask: What is Baroque? To this the author has no answer, beyond the statement that it is hard to define and that it is difficult to draw a line between the Renaissance and the Baroque. This brings us to the heart of the problem. Sigerist, in 1928, used the concept of the Baroque to illuminate Harvey's work. Since then the appellation Baroque has been increasingly applied, but there is no clear analysis of what is actually meant by Baroque in medicine, or how one can usefully employ the concept. In a sense, the problem is no different than that of Romantic medicine. Here, too, a concept derived from literature is applied to other fields of endeavor. Nonetheless, a relatively clear and useful denotation of the term can be achieved (see George Rosen: Romantic medicine. A problem in historical periodization. *Bull. Hist. Med.*, 1951, 25, 149-58). A similar approach to the problem of the Baroque in medicine is needed, and I am at present preparing an analysis of this concept, how it may be understood and how it can be used for studying the history of medicine. For this purpose as well, Karcher provides useful material, even though his ideas on the Baroque are, to say the least, quite unclear.

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